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THE PRINCIPLES OF CAUSALITY[1]

1. INTRODUCTION

A STATEMENT of causal law, in the restricted sense in which I propose to use the term, serves to explain, and predict or control, a mechanical process. The statement is to the effect that the initial state of physical movement causes the subsequent state in accordance with the relevant law of mechanics. The question is: What does the word "causes" in a statement of the form "C causes E" mean? The answer, which is the principal contention of this essay, is that the meaning is derivable from a personal experience of causal compulsion in the form of an experience of a tendency or disposition.

A causal law is not to be identified with a law of mechanics (or, more generally, with a law of physics). A mechanical law forms a part in the complete formulation of the causal law: the specification of the cause, i.e., of the initial state of a particular mechanical process, enables the physicist to apply the relevant law of mechanics in order to compute or predict the effect, i.e., the subsequent state within the same process. Accordingly, a law of mechanics is the causal connection between the cause and the effect and must be supplemented by an explicit reference to the interconnected states in the statement of the corresponding causal law.

The prevalent confusion between a causal and a physical law is a consequence of the physicist's failure to give an explicit statement of causal laws. Yet there can be no doubt that physics is concerned with the latter. The conclusive evidence is negative. The quantum physicist does not intend the breakdown of physical laws when he announces the breakdown of causal laws; he means the impossibility of specifying the cause, i.e., of specifying simultaneously the position and momentum of a moving particle, and the ensuing inapplicability of the mechanical law. If positive evidence is desirable, there is the following general consideration. Physics is expected to enable the physicist to predict, and control, the course of nature. Neither the specification of a state of the

[1] This is a revision of a paper read as part of a symposium on "The Conception of Law in Science" at the 48th annual meeting of the American Philosophical Association, Eastern Division, at Bryn Mawr College, December 27, 1951.

course alone nor the statement of physical laws alone is sufficient for the required control and prediction. The physicist needs both the knowledge of the state and the knowledge of physical laws. But the conjunction of the two is exactly what a causal law requires.[2]

The justification for using the term "causal law" in the present sense depends upon two theses. First, explanation is essential to science, and, second, causal compulsion is a principal source of explanation. There are other explanatory factors: generality, simplicity, systemic correlation, among others. But physics without causal laws is—to use once more the simile because of its aptness in the present case—like *Hamlet* with the part of the Prince of Denmark left out. Quantum physics is a case in point. Generality, predictive success, and the like, are all there. But leading physicists are concerned with its lack of explanatory virtue. Witness Einstein's dissatisfaction with the absence of causal explanation. And Bohr attempts to do justice to the causal or dynamic aspect of nature by defining it as one of the pair of complementary aspects.

To show that causal compulsion is explanatory of mechanical manifestations let me begin with personal experience. "The movement of my body was caused by a push (or pull)" and "My body moved because of a push (or pull)" are two singular statements of causality to describe the same personal experience. The fact that the word "caused" of the first statement gives way to the word "because" in the second statement testifies semantically to the presence of explanation in the experience. Accordingly, a statement of causality is acceptable, or said to be true, provided it explains physical movement in terms of push or pull. Suppose, for example, that I happen to stagger in a crowd. My companion asks me: "Why did you stagger?" I answer: "Because I was pushed." The answer is satisfactory to both of us. No one except

[2] In his article on "Causality and Necessity" (this JOURNAL, Vol. XLVI, 1949, pp. 257–270) Mr. Hofstadter would seem to be dealing with causal laws in my sense of the term. For he identifies causal necessity with the exclusion of the logical possibility for an individual *a* to exemplify the cause-characteristic S but not the effect-characteristic P. This is to say that in addition to the correlation of physical characteristics (which is given by a law of physics) he needs reference to the states of *a*. On the other hand, in his original contribution to the present symposium Mr. Hofstadter would seem to identify causal laws with physical laws. The fact that in the article the assertion of causal necessity is said to be synthetic whereas in the symposium paper Mr. Hofstadter says that he would prefer "to take the statement, 'Given the cause, the effect occurs,' as analytic . . ." is, in my opinion, evidence for a shift of meaning, whether intentional or not, in Mr. Hofstadter's treatment of causality.

an ill-advised philosopher would persist with the question "Why should the push explain the subsequent movement?" The common-sense answer to the philosopher is: "Because it does; because it provides intellectual satisfaction or understanding."

The root of explanation by causal compulsion is that the explanatory terms are agencies or factors (a push or a pull) that in addition to magnitude have direction; in short they are tendencies or vectors. We understand or explain a physical manifestation provided we recognize it for what an antecedent tendency or vector was directed to. Consider, for example, spontaneous movement. Suppose I want to move. The awareness of the intention is an experience of a tendency. Therefore the subsequent movement is recognized for an execution of the intention or a manifestation of the tendency. There is no denial that the existence of a tendency explains the fact of its manifestation. Similarly with a push or pull: it explains subsequent movement because in personal experience it is felt to be directed towards its outcome. There is no feat of the imagination in extending explanation in terms of local push or pull from the field of conscious experience to the field of physics. For it is not the presence of consciousness but of directedness that makes for explanation in terms of tendencies.

How far can explanation in terms of local push or pull be carried through in physics? Not very far in classical physics, since the latter is based upon Newtonian action at a distance. We may remember that this shortcoming was a matter of concern to Newton himself. But the vector fields of relativity physics make allowance for causal explanation, i.e., for local causation, without restriction. Let me suggest that the difference with regard to the part assigned to causal explanation may be one of the reasons behind the readiness with which physicists have abandoned Newton in favor of Einstein. Let me also point out that Einstein intends his physics to be inclusive. And if his program will be sometime realized the proposed conception of causal law may turn out to be not at all restricted or narrow.

However, there may be physical laws that are not reducible to mechanical laws and therefore not amenable to causal explanation in terms of push or pull. Let me grant the possibility for the sake of argument. Even so I should contend that causal laws *lend* a measure of explanatory efficacy to their recalcitrant associates provided the association is a partnership, i.e., laws of both kinds coöperate in the teamwork of physical theory. In other words, I assume a kind of diffusion of explanation through contextual or systemic correlation among different statements of law within the framework of physics.

Explanation in terms of causal compulsion assures continuity between personal experience of bodily movement and pre-scientific causal statements, on the one hand, and causal laws of physics, on the other. But the continuity is not unqualified. For explanation in physics requires unconditional generality and a degree of precision that enable the physicist to predict. This means that causal explanation is not the same thing as prediction. The differentiation may appear objectionable. One may argue that to say that the cause explains, or accounts for, the effect is to say that the knowledge of the cause should enable us to anticipate the effect. But the objection is a mistake. To answer it we need only reëxamine the kind of personal experience that yields causal explanation. In the case of spontaneous movement the operative intention may suffice for prediction. But with the more relevant personal experience of motion caused by push or pull explanation is retrospective and not anticipatory. I explain the movement of my body, but only *post factum,* by reference to the antecedent cause. In physics, on the other hand, there is the additional knowledge of the physical law to tell the physicist how the cause operates in bringing about its effect. For to let him know the way of the cause is to enable the physicist to figure out what exactly to expect from the effect. Let me consider the matter further with the aid of a statement of causal law.

The subjunctive conditional is an appropriate form for a statement dealing with dispositions or tendencies. For example, to elaborate on the fact that an unbroken glass is breakable, i.e., has a disposition to break into pieces, we might say that if it were dropped on the hard floor the fragments of glass would scatter around. Since a cause has been defined in terms of tendencies or dispositions a similar form for a statement of causal law is in order. Let us try the following formulation. "If a definite state of motion were to take place at a given moment, then—in the absence of interference—another definite state would take place at the next specifiable moment in accordance with a law of mechanics, i.e., with a formula of correlation between antecedent and subsequent states of a mechanical process." In this formulation the antecedent state is to be identified with the cause and the subsequent state with the effect. The motion of a particle with a mass m along a straight line x in the field of a constant force F may serve as an illustration. The formula of causal correlation is Newton's second law of motion, i.e., the differential equation "$F = mx''$ ". The cause is the initial state of motion at the time t_0, the effect is the subsequent state at the time t_1. To predict the effect the physicist integrates Newton's law of motion to obtain

the equation "$x = 1/2\ Fmt^2 + at + b$," where a and b are arbitrary constants of integration. The replacement of a and b by the values x_0 and v_0 (the position and velocity of the particle that determine the initial state of motion) yields the equation "$x = 1/2\ Fmt^2 + v_0 + x_0$" that enables the physicist to compute the effect by calculation, i.e., to determine x_1 and v_1 of the state at t_1.

The formulation of a causal law by means of a subjunctive conditional brings out clearly the differentiation between explanation and prediction or, which amounts to the same thing, between the operative tendencies, i.e., the cause, and their way of operation. Let me add that the differentiation in question makes us describe the way of operation as if it were another disposition in addition to the operative tendencies themselves. And with this observation I am confronted with the objection that the form of the subjunctive conditional, the "if-then" form, that is, serves to describe the disposition of the way of operation rather than, as originally intended, the operative tendencies. For, according to the objection, the reference to the operative tendencies is confined to the antecedent, to the "if" clause, of the subjunctive conditional.[3] My preliminary answer to the objection is that the reference to the cause is not confined to the antecedent clause because—except for the purpose of analysis and description—the cause is not to be dissociated ontologically from the way of its operation. Even on the plane of analysis and description the differential form of the laws of mechanics serves to hook up the disposition expressed by the subjunctive conditional as a whole to the tendencies mentioned in its antecedent clause. The point is that, as Russell has repeatedly and emphatically stated, the form of differential equations of physics does not tell what happens but what is *tending* to happen.

But if ontologically the cause is not to be dissociated from its way of operation why do I identify the cause with the antecedent clause of the subjunctive conditional and not with the subjunctive conditional as a whole? The answer is that the association of the tendency with its way of operation is a relation of exemplifying which is not identity. This is to say that the initial and the subsequent states within a process of motion may consist of different tendencies—both the direction and the magnitude of constituent vectors may change with the changing phases of the process—that yet exemplify the same way of operation. The invariant way may be said to represent the trend in which every constituent tendency, causes and effects alike, participates. In relation to the trend the associated tendency operates as if it were a drive towards

[3] This objection I take to be a substantial part of Mr. Hofstadter's criticism of my symposium paper.

the preservation of the trend throughout the evolution of the physical process. Accordingly, the manifestation of a tendency —towards which the tendency is directed—is nothing short of the total process, i.e., of the causal chain of successive states, under the control of the dominant trend. But taken by itself a tendency—except when it happens to operate in the mode of an intention—cannot be aware of the inclusive process towards the completion of which it happens to lead. In fact, in order to preserve the trend, and carry it over to the next link of the causal chain, the tendency can do no more than tend to persevere. But if the tendency agrees with the associated trend it does not have to do anything else. Hence, taken in isolation from the process of motion, a tendency can be defined as a tendency to be continued, i.e., as a tendency towards self-preservation. Newton's first law of motion follows immediately as a consequence of that definition. Why then does it happen that subsequent tendencies within the same process fail to duplicate, i.e., to continue with the magnitude and direction of, the initial tendency? In terms of physics, why does the physicist need Newton's second law in addition to the first? The answer is that, under the circumstances of a changing environment, tendencies must be replaced by different ones and not continued in order to carry through the same trend from one phase to another in the evolution of motion. Newton's first law would be applicable in the environment of an empty Euclidean space in which the tendency of the moving particle to preserve the same speed and direction would be realized through the course of uniform rectilinear motion. Under these circumstances, the trend and the tendencies would be indistinguishable. But in the actual world, with other physical bodies in the surroundings, the environment of a moving particle is not Euclidean but a space of variable curvature. And if space is not flat the invariance of trend requires the associated tendencies to change their directions in the course of motion. For example, if the course of a ship is due north, the navigator must take into account that the ocean is laid on the surface of a sphere and plot the course along the curve of the meridian and not along a straight line. The implied differentiation between the trend, or the course of motion, and the infinitesimal segments, or constituent tendencies, along the path of the course cannot be established, however, within the span of a single segment but requires observation of a length that is sufficient to bring out the curvature of the path. In other words, a certain amount of evolution within the process of motion must take place before we can distinguish between the trend and the associated tendencies. And since a cause is generally understood to *start* the causal chain,

or the process, going, the reason for not identifying the cause with the trend is evident.

In the above analysis of a causal situation one may choose to identify the cause either with the initial tendencies or with the prevailing trend or with the change in the environment. I have just explained why I do not identify the cause with the trend. But the derivation of causal explanation from personal experience of push or pull may be construed as favoring the identification of initial causes with external forces. I do not think, however, that such an interpretation would be correct. Do not let us forget that push and pull are local forces that are operative because of being assimilated by bodies that are pushed or pulled. Nor must we forget that, according to Einstein, the field of local forces is equivalent to the environment of curved space. And if we follow Einstein, we would not want to say that space causes motion. It would be much less awkward to say that the curvature of space conditions the change of motion. The truth is that the concept of curvature enables the physicist to employ a generalized and modified form of Newton's first law to describe the motion of a particle whether the environment is flat or not. The physicist tells us that a body *tends* to continue with its state of motion by following a geodesic. To quote A. D'Abro:

> Thus, when we wish to determine the path and motion, say, of a planet, we first determine the space-time curvature around the sun and we deduce therefrom the lay of the geodesics (straightest lines). The world-line of the planet will then lie along the geodesic corresponding to the initial position and velocity of the planet. When the geodesic is known, the path and motion of the planet are obtained. This is what is meant when we say that the space-time structure guides the planet.[4]

To guide is not to cause the motion of the planet. The cause is given through the specification of the initial position and velocity of the planet to which a particular geodesic corresponds.

The distinction between the factor of explanation and the factor of prediction in a causal law is a matter of concern for epistemology and semantics. The physicist has no regard for personal experience of causal compulsion but resorts to scientific method and technique, including the procedure of inductive reasoning, in order to establish the laws of motion and, in particular, the field equations which determine the geodesics. But with causal explanation, which endows the terms "cause" and "causal connection" with meaning, the case is different. For both the meaning and the truth of causal statements rests upon personal experience of com-

[4] *The Decline of Mechanism*, New York, D. Van Nostrand Co., 1939, p. 85.

pulsion. The meaning, because—as Hume has demonstrated—apart from its transaction with the observer the physical world does not present anything identifiable with a causal connection. The truth, because, in the absence of evidence to the contrary, causal explanation that goes with an immediate experience of compulsion is enough to warrant, or make acceptable, the corresponding statement of causality. This is to say that immediate recognition of explanatory efficacy enables us to assert provisionally that the statement is true.[5] In relation to each other truth is the function of explanation and not the other way around.[6]

To derive the explanatory meaning and truth of causal laws from personal experience of compulsion is to generalize from single instances. Yet, as I have already mentioned, scientific induction is ruled out as a method of generalization in the present case. Under these circumstances the proposed derivation may appear to be objectionable.[7] In order to justify my position I shall proceed to show that causal connection is, in a sense, a necessary connection. For necessity entails generality. The principles to specify the sense in which causal connection is necessary are to be introduced under the name of the principles of causality. The remaining parts of the present essay are concerned with the statement and justification of the principles of causality.

2. Causal Necessity and the Principles of Causality

Causal connection is necessary in a restricted or qualified sense. The qualification is to the effect that, under specifiable circumstances, the occurrence of the cause may not be succeeded by the occurrence of the expected effect. On the premise that the cause is a tendency or disposition the reason for the qualification is obvious. For a tendency may be counteracted by another tendency to prevent the happening of the intended or expected effect.

[5] The word "provisionally" has been inserted in the text in order to leave open the possibility that an explanatory statement, which is acceptable intrinsically, i.e., on the grounds of personal experience, should be unacceptable systemically. The explanatory statement may be disregarded, or even rejected, if it proves to be unadjustable within the system of human knowledge or science.

[6] The differentiation between causal and physical laws—and the attendant distinction between explanation and prediction—were not sufficiently emphasized in my symposium paper to prevent my critics from the misdirected criticism according to which personal experience is incapable of providing knowledge of the specific form of physical laws. I did not mean to contend that the science of physics is derivable from personal experience.

[7] In his symposium paper Mr. D. C. Williams has contended that generality is an ultimate factor of scientific explanation, not to be reduced to the completely individualized set-up of a personal experience.

And the specifiable circumstances—mentioned in the statement of the qualification—are the agencies of interference. But even if the above premise is not granted, the notion that causal necessity is unconditional, or that the occurrence of the cause makes the occurrence of the expected effect inevitable, can be readily shown to be false.

The truth is that causal inevitability is contrary to observable facts. Observation of nature discloses that the failure of the expected effect to occur after the occurrence of the cause is common and frequent. You may be expecting an inadvertently dropped morsel of food to hit the ground when your dog snatches the morsel in the air. The dentist's drill fails to cause the feared pain if he uses anaesthetics. And, as this example shows, we often counteract the cause in order to prevent the occurrence of an undesirable effect—a practice that would not be possible if, given the cause, the effect were inevitable.[8]

The doctrine of inevitability, however, has unquestionably an element of truth. In fact the statement of inevitability, "Given the cause, the effect is inevitable," can be treated as an incomplete locution, ". . . given the cause, the effect is inevitable," which yields, upon completion, the truth that "Under favorable circumstances, i.e., in the absence of interference, given the cause, the effect is inevitable." For, as the examples of the preceding paragraph show, the reason why the effect fails to occur is always some interference with the cause. Let me add that, on the premise that the cause is a tendency and the effect the manifestation of the tendency, the inevitability of the effect of an unopposed tendency follows analytically or tautologically from the definition of the terms.

So far I am in agreement with Hume when he points out that the principle of the association of the ideas of cause and effect "is not to be considered as an inseparable connection . . . but as a gentle force, which commonly prevails. . . ."[9] I think that Hume's gentle force is what I mean by a tendency. But in the subsequent development of the argument Hume shifts his grounds to interpret causal connection, in a version of the doctrine of inevitability, as unconditionally necessary. The shift is a consequence of his using only two principles of causality, viz., (1) Every event must have a cause, and (2) Same cause, same effect, in the

[8] In his "Causality and Necessity" Mr. Hofstadter holds that the logical possibility that the individual *a* exemplifies the cause S but not the effect P is ontologically "not-allowable." Would the statement "If *a* is acted upon by a force F, it is not allowable for *a* not to start moving with an acceleration that conforms to Newton's second law" be an example? If so, the article endorses causal inevitability and is subject to my criticism.

[9] *A Treatise on Human Nature*, Everyman's Library, 1911, I, p. 19.

analysis of causal connection on a tacit assumption that they complete the set of relevant principles. For the conjunction of (1) and (2), unqualified by additional principles, is equivalent to the doctrine of causal inevitability.

To avoid the mistake of Hume's shift of meaning let me try to complete the list of the principles of causality. There are at least five principles to be considered: (1) Every state or event must have a cause; (2) Same cause, same effect; (3) Same effect, same cause; (4) Every state must have an effect; and (5) The principle of isolability to the effect that interference with a causal process can be practically eliminated.

Without the principle of isolability only (1) and (4) would be acceptable. For suppose there is an interference with the process. The emergence of a certain state within the process—the interference notwithstanding—can only mean that, in fulfillment of (1), the cause of the state has prevailed. And the non-occurrence of the expected effect upholds (4) so long as the given state causes some different effect, for example, in the form of resistance to interference. On the other hand, the conjunction of (4) and (2) is not permissible because the effect in the form of resistance to interference is different from the effect that would follow the cause without interference. Finally, we can easily demonstrate the breakdown of (3) but we must qualify, i.e., we can support the plurality of causes with an abundance of illustrations but only if we do not leave the level of macroscopic presentation to undertake molecular analysis. For example, a material body may be in a state of rest at a given place and time either because it was not moving to begin with or because it was brought to a standstill by force. However, to say that the state of rest is the same in both cases is to disregard further analysis which presumably would reveal that molecular states in the case of prolonged rest and in the case of arrested motion are different.

The dependence of (2) and (3) on (5) makes us recognize the basic part of the principle of isolability in the analysis of causal connection. Principle (1), Every event or state must have a cause, is equally important. For, as I shall proceed to show, principle (1), taken in conjunction with the principle of isolability, enables us to deduce the remaining three principles on the list.

The proof of (2), Same cause, same effect, is surprisingly simple. Suppose that an unusual or different effect follows the recurrence of the same unobstructed cause. On this supposition the difference between the usual and the unusual effect would be—in violation of principle (1)—an uncaused manifestation. Hence principle (2).

To prove (3), according to which there is no plurality of causes, let us observe that the possibility of alternative causes of the same specific state would imply that no cause is specifically relevant to what happens to be its effect. But the rejection of specific causal relevance would allow, in principle, any state or event to cause any other state or event. And if any cause can bring forth any effect, then—contrary to principle (2)—the same cause can at one time bring about one effect and at another time a different effect. But principle (2) has been already proved and therefore must not be violated.

The proof of (4), Every state or event must be a cause, involves a difficulty. We begin the argument by pointing out that, if M and N are two successive states within the same causal chain or process, then M must be the cause of N because if it were not N would be—contrary to (1)—an uncaused occurrence. But in order to point that out we disregard the possibility of a remote cause, i.e., the possibility that some state earlier than M is the cause of N. The difficulty is removed by explaining why the possibility of a remote cause is to be disregarded. The proposed hypothesis which equates physical causation to local compulsion by push or pull entitles us to reject causal action at a distance whether distance be remoteness in space or in time. I conclude that (4) is also deducible from (1) and (5).

The reduction of the list to two principles means that an examination and justification of these two is in order. Let me begin with the principle of isolability.

3. The Principle of Isolability

The principle of isolability is the proposition that causal physical processes evolve, or can be helped to evolve by protection from interference, each in accordance with the relevant law of physics regardless of what may happen elsewhere. The first step towards a theoretical justification of this proposition is a proof that at least the initial cause of a process under examination is observable or specifiable without interference from outside. Even this first step could not be taken in classical physics. For a physicist who admits instantaneous action at a distance cannot know what he is talking about in referring to the cause under observation or specification: the object of reference may be in an unaccountable state of alteration under the action of some indefinitely distant and unknown cosmic cataclysm at the very moment of the attempted reference. The possibility of such instantaneous interference from outside has been ruled out with the advent of relativity physics. The initial cause can now be identified with the world-point O which is the

apex of the light cones in the familiar space-time diagram, and this identification means that nothing outside the cones, and therefore no contemporary agency, can have a causal influence upon the event at O. But to say that the cause O is completely isolated from interference is not the same thing as saying that the world-line to which O belongs must exemplify some single well-established physical law so that any event subsequent to O but on the same world-line is not at a point of intersection with other world-lines and therefore not an outcome of many concurrent or conflicting causal chains.

We should have fully justified the principle of isolability if we could treat the total process, i.e., the history of the world as a whole, as a single causal chain. For there is nothing outside the world and therefore nothing that might interfere with the world-wide process. A momentary cross-section of the process would be a world-wide state identifiable with a cause, and an exhaustive knowledge of positions and momenta of constituent particles—a knowledge within the power of a Laplacian demon—would enable one to predict the effect, i.e., the specific nature of a subsequent world-wide state. But since we are not Laplacian demons the treatment in question is altogether impracticable. The idea, however, has a semantic value because it helps us to restate the principle of isolability with greater precision. The principle of isolability stipulates that states within a particular causal process can be treated as if they were world-wide.

Reference to world-wide states indicates that the principle of isolability is an idealization. Therefore the justification of the principle requires—in accordance with the scientific way of justifying an idealization—that the actual treatment of a particular causal process be amendable through a series of successive approximations to the inclusive or world-wide ideal. And in this sense of justification the principle of isolability would seem to be entirely justified. For suppose that there is interference where none was to be expected. This may be a statistically discountable accident as when, in spite of all laboratory protection, an experiment does not come through successfully because the scientist inadvertently dropped the test tube on the floor at the critical moment. As a matter of fact many accidents of this kind can be eliminated by further refinement of protection. For example, the floor of the laboratory can be padded so that the dropped test tube would not break. On the other hand, unexpected interference may turn out to be systematic and unamenable to laboratory control. If this should happen the physicist can always assume that his formulation of the physical law which controls the apparently unruly proc-

ess is inadequate and try another. The ideal of an inclusive process dictates the form of a closer approximation. The new formulation of the law must include reference to the factor of interference in the form of an additional parameter. Of course, the addition of parameters must not bring out a complexity that would make the law too unwieldy to be expedient. Rather than overstep the boundary of expediency the physicist can always uphold the principle of isolability by blaming the present technique of protection from interference on the assumption that the technique is subject to improvement. Let me sum up by saying that the principle of isolability is valid because the physicist has a choice between two complementary procedures of validation. When a causal process appears to be under laboratory control he relies on the simplest formulation of the relevant law of physics, with the number of parameters at the minimum, and removes the factors of interference that are not negligible outside the controlled field of observation. On the other hand, when facilities of control are unavailable he extends the field under observation by including every recalcitrant factor within it. To illustrate the second procedure, take the gunner who has under control the two basic factors of the explosive energy of the shell and of the angle of inclination of the barrel of the field piece. If the application of the relevant simple formula proves the artillery fire to be inaccurate, the gunner resorts to a more complicated formula that takes care of such uncontrollable factors as the strength and direction of the wind.

The justification of the principle of isolability means that all causal processes are isolable. In the section to follow there is a proof that an isolable physical process is causal. Can we go a step further and contend that every physical process is an isolable or causal chain? There is the possibility of unavoidable interference with certain physical processes in and through the technique of observation and experiment. But this possibility means that the isolability of such processes is unobservable rather than that they are not isolable. Let me take advantage of this consideration in order to question the alleged breakdown of causality in the field of quantum processes. For is not the inevitable disturbance of a quantum process, for example, of the movement of an electron, by the instrument of observation at the root of apparent causal anomalies? At any rate it would seem to be so if we follow the interpretation of Heisenberg and Bohr. According to their account, any instrument designed for the specification of one of each pair of physical complementaries, such as position and momentum, would inevitably interfere with the simultaneous specification of the other. But then what happens is no more than an inevitable

substitution of a complex causal situation for a simple one: the intended process in which one state of the moving electron causes another happens to be replaced, in the act of observation, by a causal transaction between the electron and the proton which is a means of observation. I cannot see why a substitution of this kind should be said to mean the breakdown of causality.

Let me complete the discussion of the present topic by a statement of what I take to be the philosophical significance of the principle of isolability. Nature is to be analyzed in terms of isolable, i.e., mutually independent, causal chains. Accordingly, the philosophy of events gives way to the philosophy of processes. I do not mean that the philosophy of events denies the existence of particular processes but that in attempting to pulverize the latter into a discrete sequence of events, i.e., of ultimate quanta of time, it raises unnecessary difficulties. One of these difficulties is a version of the traditional puzzle concerning causal connection. If cause and effect are discontinuous quanta of time they are disconnected fragments, i.e., there is no causal connection: while the cause is operative the effect to be operated upon is not yet in existence and when the effect appears the cause is already gone. The philosophy of process, on the other hand, provides no grounds for such puzzles. In particular, causal connection is, as I have already explained, at once in both the cause and the effect, and therefore manages to tie them together, in the sense in which the same trend is present in different sets of tendencies that make up different states of the same process. The unity of the trend is the integral unity of the process and its states are phases of its continuous evolution and not discontinuous fragments of time.

4. The Problem of Uncaused Events

Let me analyze a particular physical process or causal chain into a sequence of momentary states according to the following schema:

....ABCDEF....MNL....

Each capital letter represents a distinct state of simultaneously specifiable conditions. For example, A may stand for the position and velocity of a moving particle at the moment t, in which case B stands for the position and velocity of the same particle at the next moment t_1. Accordingly, A represents the cause of B. The letters of the schema form a discrete series. This is not intended to mean that the represented process is not continuous but only that the selection of humanly specifiable states out of the total process yields a discrete sequence. In particular, the appearance

of B next to A represents the fact that the effect is the state which is specifiable, with the best available technique of observation and experiment, next to the cause A. The presence of dots symbolizes the states that have been left unobserved or unspecified. The acceptance of the principle of isolability entitles us to treat each of the letters as if it stood for a world-wide state.

Suppose now that an attempt has been made to reject the principle that every state must have a cause. The attempt is equivalent to the contention that some state—for example, the state B—may be uncaused. But to contend that B may be uncaused, i.e., that the occurrence of B may be unconditionally contingent, is to assert that a state different from B might have occurred between A and C. Let us consider the meaning of the word "might" in the assertion. After the occurrence of B, which has been already granted, the fact cannot be undone or erased from reality. And the principle of isolability rules out the translation of "B might have not occurred" into "Under different circumstances, i.e., in the course of interference, B would not have occurred."

The notion of a contingent B could mean a commitment to the ontology of unrealized possibilities. But a statement which asserts an unrealized possibility has the form of a subjunctive and contrary-to-fact conditional. For example, in the case of B, we might say: "If the tendency towards the occurrence of B had not prevailed, a different tendency would have brought forth instead of B a different state." But in this form the statement of the possibility of an alternative state is incompatible with the rejection of the principle that every state must have a cause. For to refer the possibility to a different tendency is to observe that a state which would be different from B would have a cause which is different from the cause of B. The attempt to speak with meaning of an uncaused B is therefore self-defeating—it ends with a reference to tendencies or causes.

I can think of only one other interpretation of the sentence "B might have not occurred." If a recurrence of A is not followed by a recurrence of B, then we can argue that B might not have occurred in the first place in the sense that an occurrence of A is equally compatible with either an occurrence or a non-occurrence of B. But this interpretation raises the question of exact recurrence, i.e., of recurrence in each and every detail. For if recurrence can be only approximate, the difference between the original cause and the duplicate, however minute, would be held accountable for the difference of effects. On the other hand, since we have accepted the principle of isolability, we must identify

the hypothesis of exact recurrence with the hypothesis of total recurrence. We may as well ask: Is it possible that the history of the world should repeat itself?

Russell has recently proved that the hypothesis that history is cyclic cannot be even meaningfully stated in terms of traditional philosophy.[10] I accept his proof. But he thinks that he can restate the hypothesis with meaning on his own terms. I shall argue that he cannot. And if my argument is correct, the conclusion is that the phrase "exact recurrence of states" is altogether without meaning.

Russell attempts to state the hypothesis of total recurrence in terms of a novel theory of particulars. Instead of following the traditional opposition between a particular instance, on the one hand, and the complex of universal qualities (which the instance may exemplify in common with other instances), Russell proposes to identify a particular with a bundle of specific qualities. With the proposed identification recurrence is no longer, as tradition would have it, an exemplification of the same universal qualities by numerically distinct instances but the presence of the same particular, i.e., of the same bundle of specific qualities, within two different contexts. To assert, with the aid of our schema, that B recurs with N, and that therefore B and N are indistinguishable, is, according to Russell, to say that B is literally identical with N although the set of B flanked by A and C must be distinguished from the set of B (alias N) flanked by M and L provided M is different from A or L from C. The proviso, i.e., the requirement for a difference of context, is essential in order to square Russell's theory with the facts of approximate recurrence. The question is: Is the requirement for contextual difference compatible with total recurrence? My answer is "No." For if an observer within the world-wide state B should establish that B is between A and C, he (or his recurrent self) within N cannot fail to establish exactly the same thing for N since, by Russell's hypothesis, N and B stand for literally one and the same world-state. And if the observer who finds that C and L are different were an observer within L, then B and N would not be identical because the observer within N, unlike his prototype within B, would be wrong in asserting that the next state is C.

Suppose, however, that I have made a mistake in the course of my argument, and that the hypothesis of exact recurrence is meaningful. Suppose, for example, that A recurs under the name of M, but let N be different from B. The supposition enables us to

[10] *An Inquiry into Meaning and Truth*, New York, W. W. Norton & Co., 1940, pp. 117–126.

assert that N is uncaused. But if we do so we commit ourselves to the notion of absolute time. Let me elaborate. The records show an invariable sequence from a recurrence of A to a recurrence of B. And since M is a recurrence of A, the occurrence of N instead of the expected B at the moment t singles out t as an absolute moment. For we can say: "*The* moment has come for N to occur instead of B." Or we might observe: "The only difference between A and M—that accounts for the occurrence of B after A but not after M—is that the date of A is not the date of M." Or, following Hume, we may remark that the future need not resemble the past and therefore imply that the future is intrinsically, or absolutely, different from the past. Either way of describing the situation stipulates the existence of singular, i.e., absolute moments or dates. And with absolute moments or dates we accept absolute time. But, of course, no empiricist would want to accept absolute time at any price. And therefore he would not want to admit the possibility of uncaused events if he knew that the admission leads to absolute time. Hume, in particular, has a relational theory of time and, therefore, his allowance for uncaused events is an inconsistency. Note that a physical law, in the form of a second order differential equation, does not mention the time explicitly. If the import of the law were an explicit function of the time, the existence of absolute time would be a fact of nature. Accordingly, the physicist needs no other justification but a rejection of absolute time for his way of formulating a law of nature. To conclude: if the notion of absolute time is objectionable, the principle that every event must have a cause is valid.[11]

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UNIVERSALITY, EXPLANATION, AND SCIENTIFIC LAW[1]

1. Mr. Ushenko's argument, as I follow it, runs somewhat like this. The function of science is not limited to description and prediction. Explanation, involving something more than either description or prediction or both, is essential to science. Now by

[11] The statement "Every event must have a cause" is preferable to "Every event has a cause" because the latter would seem to allow for the possibility—even if not for the actuality—of an uncaused event.

[1] This paper was read as part of a symposium on "The Conception of Law in Science" at the 48th annual meeting of the American Philosophical Association, Eastern Division, at Bryn Mawr College, December 27, 1951. It is published here in its original form. As a consequence, the references in it to Professor Ushenko's paper are to that paper in its original form, as it was read at the meeting, and not to the revised version printed above.

an explanation of an event we understand it or know why it occurs. Whence do we derive our understanding of events? Not—or at least not ultimately—, as is usually supposed, by subsuming them under unconditionally universal laws. On the contrary, laws themselves derive their explanatory power from our direct insight into singular cases of "causal compulsion," in which we ourselves are subject to or exercise such compulsion, as in being pushed or pulled or moving voluntarily. The explanatory power of these singular insights then diffuses to other cases more or less similar, and eventually throughout the system of scientific knowledge. But if we were to sever the bond between the theoretical systems of science and personal experience of causal compulsion, we would rob science of all explanatory power. In the end, presumably, it would cease to be science or knowledge, becoming at most a mere mechanism of description and prediction, if any such thing remains possible.

Also, the causal compulsion that makes scientific law a means of explanation is not the same as unconditionally necessary connection. It has to do with tendencies and their manifestations, not with some sort of unexperienceable and absolute tie between events. Hence, Mr. Ushenko thinks, we must re-examine our conception of causality so as to make it adequate to the truth of the matter. Usual notions, such as that the cause necessitates the effect, or even that the cause is always accompanied by the effect, must give way to more refined notions such as his "principle of isolability" and the principle that every "isolated event" must have an "isolated cause."[2] As a result of all this, then, we are to emerge with a new, and true, view of explanation, scientific law, and causality.

Assuming that this is a fairly correct presentation of his view, I shall now try to explain why I think that his theory of scientific explanation and law is inadequate, as a result of which his proposed reformulation of causality is superfluous; and I shall add a few words about the question whether scientific law includes an element of necessity over and above unconditional universality.

2. It is important first to get a clearer view of Mr. Ushenko's conception of the nature of explanation. What, according to him, is it to explain an event? Scientific laws are supposed to derive all their explanatory power from personal experiences of causal compulsion. Hence we must turn to these experiences to find the

[2] I suppose that the "must" here carries the sense of "compulsion" rather than the classical sense of "unconditional necessity," although the compulsion in cases of isolated processes may be exceptionless.

ultimate nature of explanation. There are a couple of passages in Mr. Ushenko's paper which are of interest on this score. He says:

> . . . awareness of an intention to move is an experience of a tendency. Therefore, the subsequent execution of the intention, the movement, is experienced as a manifestation of a tendency. And there is no denial that the tendency explains, or accounts for, the fact of its manifestation. Now a tendency has an intensity or magnitude and a direction. Therefore a tendency is a vector . . . Causal laws are explanatory because of their vector components.

And again, supposing you intend to raise your hand and you do:

> Do you know the cause of the event? Of course you do. You know that the tendency to raise the hand is the cause. And you know that because causal compulsion, the realization of the intention to move, the embodiment of the tendency in a spontaneous overt act, can be directly felt.

It seems clear from his paper that Mr. Ushenko thinks the basic act of understanding consists in directly feeling the event to be explained as a manifestation of a tendency and the tendency as embodying itself in the event. This movement of the tendency from power to act is the content of a singular perceptual experience. In it we get a first-hand acquaintance with the primordial ontological flow of causal compulsion.

Let us suppose, for the argument, that his account of experience of causal compulsion is faithful to fact, and let us ask why Mr. Ushenko thinks that such an experience is an act of *understanding*. In his paper he does not deal explicitly with this question, although it is so important and basic for his entire analysis that it urgently begs to be treated adequately. He says some things, however, that suggest a viewpoint. They have to do with what he calls the "vector" character of tendencies.[3] I believe that he thinks of a tendency as "tending towards" its manifestation, as "directed towards it"; so that, if and when the latter ensues, it "completes" the process or terminates it, as reaching the goal terminates the striving towards it.

Mr. Ushenko says something like this in another place (*Power and Events*, pp. 206–207):

> . . . we are forced to recognize in our experience of power through actuality a peculiar unity in difference. Actualization is never a discontinuous transformation; a power is felt as aiming at a manifestation in a percept; and the percept is experienced with an ultimate sense of satisfaction or finality in the attainment of the aim. These are the factors in our unerring recognition of

[3] I shall not stop to quarrel with this terminology beyond noting that there are many dispositions and tendencies of which it is at least unusual to say that they have a "direction," e.g., the "fragility" of a glass, which Mr. Ushenko cites. What direction, in any usual sense of the word, does a tendency to break have? Or what is the direction of the specific heat of water?

a percept as an actualization of a certain power in spite of the fact that there can be no resemblance between a power and an actual percept.

Although this passage occurs in the context of a treatment of perception, I believe Mr. Ushenko would want to say the same kind of thing about the case of voluntary arm-movement or being pushed. Of the latter, for instance, he says that we "experience tendencies, or vectors, in transaction," that is, I take it, in their manifesting-action.

We now begin to catch a glimpse of Mr. Ushenko's theory of the basis of explanation in its connection with causality. A perception of a power manifesting itself in an act is a perception of causality in process (*Power and Events,* pp. 215–218). There is here a unity-in-difference: the power or tendency issues into what it was able to issue into; perhaps we might even say it is being itself in doing this. In apprehending this unity-in-difference we are apprehending the core of the causal relation. We *feel* the power manifesting itself, i.e., *we feel the becoming of the effect,* and in this feeling we get all we need to *understand* the outcome.

And if we ask how we know that the feeling is veridical, the answer is (Mr. Ushenko says he is here stating it simply dogmatically; and on this we agree) that in cases of this sort "subjective certainty and objective validity are indistinguishable."[4] Thus knowing is here supposed to be indistinguishable from feeling; the subjective certainty we get in and through the feeling of a passage from power to act cannot be distinguished from the objective validity characteristic of knowledge *qua* knowledge. And as Mr. Ushenko maintains, in this kind of situation "truth is the function of explanation and not the other way around."

What we feel directly, then, is a process of actualization, and hence actualization is the key to explanation.

But let us still ask: Why should Mr. Ushenko think that a feeling of a process of actualization is an act of understanding? How do we *understand* an event by perceiving it as the manifestation of a tendency?

Unfortunately, beyond what I have already quoted or similar passages we are not given an explicit account. Perhaps Mr. Ushenko thinks that once one has had the feeling of passage from power to act one knows enough not to ask silly questions. At any

[4] This seems not altogether consistent with his express position that there is a possibility, though he calls it unlikely, that "an explanation, although acceptable on the grounds of personal experience, is unacceptable systematically. If a piece of explanation should not be adjustable to the bulk of human knowledge or science, we would do away with the explanation." But I shall not press the issue here.

rate I have experienced intending to move and then moving, and being pushed and then staggering, and I still feel the compulsion to ask the question. (I ought, according to Mr. Ushenko's theory, to understand what I am doing.)

Let us, however, for what light it will throw, take a look at Mr. Ushenko's conception of "tendency." One of the examples he gives of a tendency or disposition is the "breakableness" of a glass. The typical and appropriate formulation of this, he says, is in the form of a subjunctive conditional:

> "If the glass were dropped on the hard floor, fragments of glass would scatter around."

It will be more helpful to re-express this as:

> "If the glass were dropped on the hard floor, the glass would break,"

since this shows more clearly its relation to the term "breakable." Note, then, that the stem of the word "breakable" goes into the *consequent* clause; the "-able" suffix dissolves away into the "if . . . then . . . ." What now goes into the *antecedent* clause?

In discussing the more technical laws of physics, such as mechanical laws, Mr. Ushenko calls attention to their analogy to the above statement, arguing that they also should be considered to express tendencies. Now what goes into the antecedent clause of a mechanical law, *on his account,* is the expression of the *initial state*—which he proceeds to define as *"cause"*—of the physical process concerned. Note, then, it is *not* the tendency for the initial state to pass into subsequent states—which is rather formulated by the whole statement—which occupies the place of the cause, but rather the initial state itself. If we apply this to the breakableness of the glass, it is not the breakableness (i.e., tendency to break) which is the cause of the breaking, but the glass being dropped on the hard floor. We might, if we wished, call the tendency the "ground" of the causal relation, or the "because"; but if we follow the analogy of Mr. Ushenko's reading of mechanical law, we would *not* call it the "cause."

This is not a mere verbal quibble on my part. It evidences a certain tension or shifting in Mr. Ushenko's thinking. He wants (1) to think of the causal relation as involved in passage from potency to act, thinking of the potency as an efficient cause which acts by manifesting itself; but also he wants (2) to think of the tendency as a ground of passage from one actuality (initial state)

to another actuality (subsequent state) with the first actuality as the efficient cause which acts by forcing the effect *via* the ground.[5]

And this, I am afraid, tends to confuse matters. Consider the case of "intention to raise the hand" and "raising the hand." Now, as Mr. Ushenko observes, whether I raise my hand, having the intention to, depends on other factors as well. I may be more inhibited than others; I may be lazy; and so on. Thus different persons are differently *disposed* to put the intention into act. The *disposition* involved here is *not the intention;* it is rather a disposition, when one has the intention, to act it out; the intention is itself *part* (not the whole) of the "initial state" or of the first part of the process.[6]

Why is Mr. Ushenko's tension significant? Because of two considerations. (1) It is doubtful whether we do perceive the disposition involved, at least in the same way in which we perceive one or more of the factors in the "initial state" or "cause."

[5] The tension shows in his language. Here are some statements drawn serially from his paper:

An "intention to move" is a tendency.

A local force, push or pull, has physical manifestations as effects.

A causes B, where A and B both stand for particular occurrences.

The cause is "the initial state that sets a physical process going"; the effect is "a subsequent state within the same process that happens to be next under observation."

In pre-scientific usage, "cause and effect are events of finite duration and not momentary states."

The initial state of a trend is "made up of tendencies."

The tendency to raise the hand is the cause.

An electric shock may cause the same overt effect as an intention.

A singular statement of causation is, e.g., "The tendency to move is the cause of the subsequent spontaneous movement."

The push makes you stagger; and the push is a tendency.

Movement of a billiard ball is "caused by the impact of a collision with another."

The weight of the father on the seat of the teeter-totter causes the child to rise.

The dentist's work causes (or fails to cause) pain.

"Every event or state must have an effect."

Stopping of the beating of the heart is a cause of death.

A world-wide cause may be conceived as a "momentary cross-section of the total process."

An event O, at the intersection of two light cones, is an isolated cause.

A momentary position and velocity of a particle would be a cause.

[6] Mr. Ushenko falls into the same trap in other instances. A push, for instance, is a "vector." But the *disposition* involved in moving when one is pushed is not the push itself but rather the disposition to move so-and-so when pushed so-and-so, i.e., one's inertial disposition (to make matters over-simple, treating oneself as a mere body).

I may be aware of my intention to raise my hand; but am I perceptually aware—in the same way—of my inhibitions, laziness, etc.? But this is less important than the following point. (2) The disposition actually involved is one which relates not only *this particular initial state* with *that particular subsequent state,* but much more, states of *this kind* with states of *that kind. There is a universal element implicit in the disposition.* Even if I were able to apprehend the disposition directly in perception, I would have to perceive not only this particular cause passing into this particular effect, but also the universal in accordance with which initial states of this kind pass into subsequent states of that kind.

It follows that we must choose one of two alternatives:

(1) In order to understand or explain this particular effect, I need only perceive this particular cause passing into it; or,

(2) In order to understand or explain this particular effect, I need to know the underlying disposition in accordance with which causes of this sort pass into effects of that sort, as well as to know that this particular cause is occurring.

Mr. Ushenko tends—by virtue of the tension in his thought which makes cause and tendency overlap or coalesce—to adopt the first alternative. He sees rightly that dispositions (or something like them) are fundamental to explanation and causality; but, wrongly, he tends to mislocate them as causes instead of grounds. It seems to me, however, that the second alternative is closer to the truth. It says that explanation involves a universal element and that we understand the particular only as we subsume it under the universal.

3. The involvement of a universal element in explanation is already present in the prescientific stage. There is a beautiful passage in Book I of the *Republic* which illustrates this. Cephalus, the successful business man (who can scarcely be scientifically sophisticated) is telling Socrates that his cronies complain about being old:

> They will tell you sadly of how many evils their old age is the cause. But to me, Socrates, these complainers seem to blame that which is not really in fault. For if old age were the cause, I too being old, and every other old man, would have felt as they do. But this is not my experience, nor that of others whom I have known. [329.]

The criterion of the presence of the causal relation is already here universal association between the two conditions.

But also for science universality is a criterion of causal relation. Science, like Cephalus, does not think it has found "the cause"

if there are exceptions. It will not rest content with a supposed explanation which nevertheless fails in particular cases. This is why Mr. Ushenko's attempt to reject "causal inevitability" is beside the point. (I shall not pause now over the word "inevitability," taking it for the present as including, as a minimum, exceptionless universality.)

How does he "prove" that causal inevitability is to be rejected? He says it is contrary to the facts. Let us consider one of his examples.

> An inadvertently dropped morsel of food does not fall on the ground—the law of gravity notwithstanding—if your dog catches the morsel in the air.

This example is remarkable. In the first place, does the "law of gravity" say that if you drop a morsel of food it will fall on the ground? Not even a primitive hunter, who drops a chunk of meat into the hands of his hungry son, believes (or would have formulated it as a universal principle) that dropped food always falls on the ground; and as for civilized men, they know it sometimes falls on one's lap.

Consider the law of gravitation. It says that any two bodies mutually exert equal and opposite attractive forces of a certain magnitude. It doesn't say the bodies will therefore collide, etc. When the gravitational force-function is inserted into Newton's second law, the resulting equation has to be integrated. Now the integration will be from, say, time t_0, taken as the initial instant, to some other moment, t_k. Which moment will t_k be? How can one tell without further information? If there is an obstacle in the way—i.e., if the force-function has to be changed at some moment—then we shall not expect that if t_k is taken *later* than that moment our prediction will be correct. If, on the other hand, we know that no other forces will be involved, we can integrate over a longer period.

Does Mr. Ushenko want to say that the law of gravitation is contradicted by cases in which objects are prevented from reaching the center of the earth because they cannot fall through more than a fraction of an inch of ground? Why does he stop at ground-level? And if he doesn't, what is the point of his alleged counter-instance?

Indeed a good deal of his discussion of "isolability" is vitiated by a peculiar view of physical laws like those of mechanics. There is not time to treat this matter in complete detail; but we should note a few points.

To be sure, one may say about a certain *contemplated* physical process that, if it starts, it need not continue or be completed.

There may be "interference," so that what would have happened without interference does not happen. But surely this does not tell us that a cause may occur without a corresponding effect. Mr. Ushenko says he suspects that those who hold for "inevitability" tacitly stipulate absence of interference. I do not agree with him. Indeed, if I thought it important to retain words like "cause" and "effect" here (and I do not), I would prefer to take the statement, "Given the cause, the effect occurs" as analytic, since I should not want to call anything a cause unless it had an effect. Thus I would take this statement to be partly definitive of cause-effect and partly prescriptive of the aims of scientific inquiry; and if analytic, it would not be necessary to add a reference to absence of interference. And I think such a view would be more in harmony with scientific thinking and practice than one that requires a principle of isolability of the sort Mr. Ushenko offers. But since words like "cause" and "effect" are so equivocal and vague, I think it better to dispense with them altogether and to turn to the analysis of scientific law directly.

4. According to Mr. Ushenko, a statement of causal law in physics has the following form:

> If a state of a certain kind were to take place, then—in the absence of unfavorable circumstances—other states of a definite kind would take place subsequently in accordance with a formula of correlation between antecedent and subsequent states of a physical process.

Having put the statement of a law into this form he is led to think one has to say, not "Given the cause, the effect is inevitable" (I would rather say, "occurs"), but "Under favorable circumstances, i.e., in the absence of interference or obstruction, given the cause, the effect is inevitable." That is, he is led to think that we need what he calls a "principle of isolability."

But as far as I can see the physicist would not state his law in this way; and I believe that Mr. Ushenko's way of stating it is not only unrepresentative of the physicist's meaning but also misleading. His reference to "favorable" or "unfavorable" circumstances does not belong in the statement, and is inserted only by neglecting the scientist's meaning.

Take the case of Newton's second law of motion. In its differential form it says that the time-derivative of the momentum of a particle depends on, and only on, the "force acting on the particle." This latter phrase is another way of saying "certain factors pertaining to the particle in its relation to its surrounding environment." The law itself is indefinite here. It does not say

specifically which factors are relevant. It only says *there are* such factors for every moving particle. Thus the equation in the law, which may be written (restricting ourselves to one dimension)

$$\frac{d}{dt}(mv) = F \tag{1}$$

is indefinitely stated. F is to be a function of certain factors, but only their existence, not their character, is asserted in the law.

The physicist customarily supposes that he is dealing with a problem in mechanics proper if F is a function of such factors as position, velocity, and time, in which case equation (1) becomes

$$\frac{d}{dt}(mv) = F(x,v,t). \tag{2}$$

In order to apply this equation to the solution of a particular case of motion, it must be integrated; and being a second order differential equation in x (taking m constant and v as dx/dt), its general solution requires in addition to the specification of the force function F, two arbitrary constants; in *some* cases the two constants are the initial position and velocity, in other cases something else. Without all this additional information, equation (2) cannot be used to solve the problem.

Suppose we now assume a certain force-function F and the necessary two constants, A and B. Our entire statement of Newton's second law then becomes:

> If a particle is subjected to a force $F(x,v,t)$, under conditions represented by A and B, then the time-derivative of its momentum equals the force.

This corresponds to the *differential* form of the law. Suppose we have solved the problem of integration, getting an equation of motion relating the position, x, of the particle to the time, t:

$$x = g(t). \tag{3}$$

The entire statement now becomes—corresponding to the integral form—,

> If a particle is subjected to a force $F(x,v,t)$, under conditions represented by A and B, then its position, x, at any time, t, is given by the equation $x = g(t)$.

It is to be noted here that the integral form of a law of motion concerning a certain system does *not* always say that the subsequent states of the system are determined by its initial state. The system has to be conservative and the states properly defined.

The above account, using position and time, is therefore more general. It represents the general form of a law of motion as it is used to describe the motion of a particle. Bearing it in mind, there are several points of interest to be noted.

(1) There is no reference, and there need be no reference, in it to favorable or unfavorable circumstances. These phrases, I shall soon suggest, are obscure ways of saying the system is conservative or non-conservative.

(2) Consider the connection between the antecedent and consequent. This corresponds to the connection between antecedent and consequent in disposition-sentences, and indicates that we are dealing with something like a disposition of the particle-environment system. The connection is between the force F and the conditions A and B on the one hand, and the occurrence of a particular motion on the other. There are two possibilities with respect to the empirical correctness of the statement if it should happen that the actual path of the particle does not agree with the predicted path:

(a) The law of motion is false; the particle-environment system does not have the disposition asserted of it;

(b) The law is true; the particle-environment system has the disposition; but the antecedent clause has not been satisfied. I.e., either the force-function has been incorrectly assigned, or one or both of the conditions A, B, has been incorrectly assigned. (Or, also, the body might not be a particle, and hence the law does not apply; the antecedent here too is unsatisfied.)

Neither of these possibilities calls for the notion of favorable or unfavorable circumstances or for a principle of isolability.

(3) In the special case in which the force is conservative, the equation of motion can be solved in such a way that the sum of the potential and kinetic energies of the particle remains constant. Hence no energy is gained or lost by the particle-environment system during the motion; i.e., the system is conservative. For such a system, if the initial state is given (the state being defined as determined by position and momentum), every subsequent state (position and momentum) can be derived; whereas this is not generally possible for the non-conservative case. From this, however, it does not follow that in both conservative and non-conservative cases the *path* is not determined, given the force-function and the conditions A and B. Mr. Ushenko wants to call the initial position and momentum the "cause" in *both* conservative and non-conservative cases; it then turns out that, in the latter, the

"cause" does not determine the "effect," which is neither surprising nor important. Why he wants to identify "cause" in this manner, I cannot say. But it is flying in the face of what physicists wish to mean in their laws.

(4) If we wish to think of the initial state (position and momentum) as the "cause," and at the same time retain the universality of causal connection, we shall say that a non-conservative system is an incomplete system and shall look to complete it by including the energy-supplying or energy-absorbing factor in the system, or by shielding the system from it, so making it conservative. "Favorable" circumstances then become "circumstances in which the system is conservative." This transforms Mr. Ushenko's formulation of the law of motion into:

> If a state of a certain kind were to take place, then, if the system is conservative, other states of a definite kind would take place, etc.

And if we define state by specification of position and momentum, then this is a provable theorem in mechanics. A conservative system may also be called an isolated system in the sense that no energy passes into or out of it during the process envisaged. It now becomes significant to say that systems are "isolable." This would mean that it is possible for there to be a conservative system. Isolability in this sense is *not* required for the validity of classical mechanics. Even if there were absolute sources or sinks of energy in the universe so that no single system was ever conservative, classical mechanics could still be true. For it does not say that there are conservative systems; it only says that conservative and non-conservative systems have different characteristics. No special "principle of isolability" is needed, and Mr. Ushenko, in introducing one, introduces an irrelevancy.

5. I wish finally to consider the question whether scientific law expresses a necessary relation that goes beyond universality. This question, though ancient, has recently been formulated in new terms. It turns into the question whether scientific law can be expressed in terms of an extensional logic or whether it calls for special modal or intensional concepts irreducible to extensional terms. Although the matter is familiar as a result of the analyses due to Goodman, Hempel and Oppenheim, Reichenbach, and others, I shall repeat certain points for the sake of the discussion.

Consider a simple lawlike statement such as "All metallic objects are electrical conductors." Expressed in conditional form it becomes: "For all x, if x is metallic then x is a conductor."

The problem then arises: What is meant by this statement? Is it simply a universal material implication:

$$(x)\,(x \text{ is metallic} \supset x \text{ is a conductor})\,?$$

It has been pointed out that this is, at least, not enough. For a universal material implication can be true simply because its antecedent clause is always false or its consequent clause always true. Hence if a law is to be expressed by a universal material implication, we must specify that it must not be true because of one of these artificial reasons. But it turns out that this too is not enough; other difficulties arise concerning the internal structure of the constituent sentences and predicates. Various methods have been proposed to meet these difficulties, though none has been satisfactorily worked to completion. Nevertheless the direction is clear. By eliminating the sources of such difficulties, we eliminate all objectionable types of sentences and emerge with only such as are acceptable as lawlike, whether true or false.

This approach to the problem of analyzing the meaning of "law" may be called the "extensional" approach, since it retains only extensional operations and adjoins only further metalinguistic criteria. Opposed to it is the "intensional" approach, according to which there is asserted to be an additional component of meaning in scientific law which is not included in the extensional account—namely, a sense of necessity irreducible to extensional plus metalinguistic terms. I once thought that the intensional approach was correct, but I no longer think so. To explain why, I shall first phrase the argument I believed compelling against the extensional approach and then indicate why I no longer think it compelling.

The argument against extensionalism runs as follows. Suppose a true universal material implication satisfies all the conditions laid down by the extensional approach. Yet it may still not be a law. For instance a statement like "$(x)\,(x \text{ is human} \supset x \text{ is mortal})$" satisfies all the criteria. Yet it might be a mere *accident* that every object is either not human or is mortal. It might be that there are certain physiological conditions under which some man might be immortal, even though those physiological conditions are never realized. E.g., it might be physiologically true that a man can be kept alive indefinitely by feeding him champagne of a certain vintage; only the vintage will soon be exhausted and we shall never know how to produce it again. Hence, although it is true that every object is either not human or mortal, there is a *possibility* that something human should be immortal. But if it was a law that humans are mortal, then there not only

would not but also there *could* not be an immortal man. Thus a lawlike statement must say not only that in fact all A's are B's, but also that there *cannot* be an A which is not B. In this way it expresses a necessary connection between A and B, whereas a universal material implication, even when purified by the extensional approach, does not.

And now for the reason why I do not consider this argument compelling. Suppose the possibility of an immortal man were in fact realized. Then, of course, the statement "(x) (x is human $\supset$ x is mortal)" would be false. If, then, we have some reason to believe *from other knowledge,* like physiology, that there could be an immortal man even though no such man will ever exist, we have reason to believe that there are certain physiological circumstances in which the alleged law "(x) (x is human $\supset$ x is mortal)" would be false. We would not then—if we believed in physiology —want to say that the statement, even though true, stated a law. But then the problem would become: What can *replace* this statement satisfactorily? That is, if we believe that a certain universal statement is only "accidentally" true, we will no longer accept it as a law, but seek to replace it by a more satisfactory statement. Now I see nothing in the nature of the situation to make it impossible to replace the above simple universal material implication by an otherwise satisfactory statement which *also* makes use only of extensional functions, but is no longer contradicted by the statement "there exists an immortal man." For instance, the new statement might be:

> (x) (x is human $\supset$ x drinks champagne of such-and-such vintage forever and lives forever or x is mortal).

Thus it appears that the distinction between accidental and non-accidental truth does not force us to admit a non-extensional necessity, and therefore does not force us into an intensional approach. We need only recognize that accidentality and non-accidentality as characteristics of universal truths have reference to some other part or parts of the body of knowledge rather than to the absence or presence of an intrinsic intensional connection between the predicates.

I think therefore that we may rest content with unconditional universality within the systematic context of knowledge and reject the need for a special intensional connection. And if so, then we may define the goal of science, so far as science seeks laws, as the achievement of universal truths concerning what always and without exception happens if circumstances of a certain sort obtain. If one wishes to call such circumstances the "causes" and what

happens under these circumstances the "effects," then science, so far as it seeks laws, seeks for cause-effect relationships where it is by definition never the case that the cause occurs without the effect. And the explanations which science gives of particular facts, and the only ones with which it will in the end be satisfied, are those which—regardless of any personal experiences of compulsion—depend on the use of unconditionally universal laws applied to particular circumstances to deduce the particular facts to be explained.

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SOME REMARKS ON PROFESSOR USHENKO'S INTERPRETATION OF CAUSAL LAW[1]

I

PROFESSOR Ushenko has maintained that "physical causation is reducible to local compulsion of push or pull" and that physical theory is explanatory or productive of understanding in proportion as it exhibits diverse cases of physical causation to be thus reducible. I wish to consider first whether modern science does, in fact, come closer to realizing Mr. Ushenko's ideal of explanation in proportion as scientists replace theories by others deemed more acceptable. And I shall endeavor to show that, far from warranting the assertion that this proportionality obtains, the evidence justifies instead the claim that the progressive theoretical encompassment of wider domains of fact increasingly goes hand in hand with an *absence* of the kind of understanding demanded by Mr. Ushenko.

Mr. Ushenko's difficulties begin within the precincts of his own mainstay, classical mechanics. Experienced teachers of physics like Lindsay and Margenau have recorded the almost *instinctive disbelief* of students in Newton's Third Law of action and reaction when applied to dynamical cases.[2] Though not failing to realize that the two forces are being applied to different bodies, I have not achieved the kind of understanding that should have come to

[1] These remarks are presented here in the form in which they were read as part of the symposium on "The Conception of Law in Science" at the 48th annual meeting of the American Philosophical Association, Eastern Division, at Bryn Mawr College, December 27, 1951. The references to Professor Ushenko's paper are to the original text of that paper read at the meeting, not to the revised version printed above.

[2] Lindsay and Margenau, *Foundations of Physics* (New York, 1936), p. 97.

me, according to Mr. Ushenko, in the dynamical push-pull case where the action and reaction are contact forces (rather than action-at-a-distance forces). For I completely fail to see *in Mr. Ushenko's sense* why the reaction is *equal* in magnitude rather than 1/10 or some other multiple of the action. I suspect that those who do claim to have such understanding possess a remarkably docile intuition. And it is the docility of our intuition in the form of dependence upon cultural and personal conditioning which makes me wonder why Mr. Ushenko thinks that he makes a point against Hofstadter by saying that in cases of insight into alleged instances of causal compulsion "subjective certainty and objective validity are indistinguishable." It will be recalled that while Schopenhauer considered it to be an *a priori* truth that only a *change* of velocity but not the velocity itself needs a cause, Galilei scarcely 200 years earlier was confronted with subjectively-certain objections, when he stated that a *constant* change of the *location* of a body is possible *without* a lasting cause.[3] It is not clear how Mr. Ushenko can reconcile the history of the displacement of the Aristotelian physics by the Galilean-Newtonian system with his contention that "there is the unlikely possibility that an explanation, although acceptable on the grounds of personal experience, is unacceptable systematically" and with his historical corollary of that claim: "whereas a law detached from experience is a prey to revision, a law that explains things is likely to survive."

If these objections are not fully convincing, I invite attention to Mr. Ushenko's treatment of explanation in quantum physics. Every student of quantum mechanics is struck by the initially awesome absence of push-pull intelligibility in its explanatory edifice. We know how futile it is, for instance, to ask for pictorial models in the Jordan-Wigner theory of second quantization. Mr. Ushenko invokes "diffusion" of push-pull intelligibility "through contextual or systematic correlation." I don't think he can deny, however, that the amount of such intelligibility which is "diffused" upward to the level of the fundamental theoretical principles is negligible, if it is present at all. Moreover, he admits that an explanation which is acceptable on the grounds of personal experience may well prove unacceptable systemically and is then legitimately rejected. Neither does he improve matters when he tells us that the isolability requirement of causality is not satisfied in experiments on the sub-atomic level so that no causal understanding in his sense can be expected in this domain. For this statement is a confession that quantum theory does not and, in

[3] Cf. R. von Mises, *Positivism* (Cambridge, Harvard University Press, 1951), p. 155.

fact, cannot achieve the understanding required by Mr. Ushenko's theory of scientific explanation! Since scientists evidently regard quantum theory as more explanatory in the subsumptive sense than classical physics, Mr. Ushenko himself has given us the best demonstration of the lack of concordance between his criterion of explanatoriness and the one employed in the practice of scientific theory construction.

It is interesting to note in passing that P. W. Bridgman is guilty of a misconstruction of explanation in science similar to the one with which we are charging Mr. Ushenko. For Bridgman also conceives of explanation psychologistically as reduction to the familiar and as aiming at bringing our curiosity to rest rather than as subsumption under comprehensive theoretical principles. Bridgman writes: "I believe that examination will show that the essence of an explanation consists in reducing a situation to elements with which we are so familiar that we accept them as a matter of course, so that our curiosity rests. 'Reducing a situation to elements' means, from the operational point of view, discovering familiar correlations between the phenomena of which the situation is composed.[4] . . . If this is accepted as the true nature of explanation, we see that an explanation is not an absolute sort of thing, but what is satisfactory for one man will not be for another."[5] Bridgman is in error, because the elements and relations encountered in the theoretical principles which explain the phenomena of nature (in the subsumptive sense of actual scientific practice) are much *less* familiar to us than these phenomena themselves.[6]

II

I now wish to turn to a consideration of Mr. Ushenko's treatment of what he calls "the principles of causality."

1. He tells us that the assumption of the existence of closed systems in nature, which he calls the principle of isolability, could not be justified in Newtonian physics, because "interference by agencies beyond the range of the physicist's control cannot be ruled out." He then goes on to assert that "successful justifica-

[4] P. W. Bridgman, *The Logic of Modern Physics* (New York, 1946), p. 37.

[5] *Ibid.*, p. 38.

[6] Bridgman even gives a psychologistic criterion for proofs in mathematics and logic. Says he: ". . . proof is a private matter. A proof presented to me with the authority of the greatest logician in the world is not a proof for me unless I can 'see' it." (P. W. Bridgman, "Some Implications of Recent Points of View in Physics," *Revue Internationale de Philosophie*, No. 10, Oct., 1949, p. 17.)

tion in the physics of relativity has succeeded the failure to justify the principle within the framework of classical physics." And to support this latter contention, he refers to the light cones of the Minkowski diagram and says: "the isolation of a cause from physical transaction with contemporary agencies and therefore [sic!] the elimination of interference, turns out to be a positive requirement within the framework of the theory of relativity."

I submit that the relativistic considerations introduced here by Mr. Ushenko are fallaciously and irrelevantly interpreted by him. For it is fallacious to suppose that the existence of a finite maximum signal velocity ensures "the elimination of interference." To be sure, the special theory of relativity tells us that perturbational influences upon the Here-Now event O can emanate only from the vast multitude of *past* events in the lower light cone instead of emanating in Newtonian fashion from all events lying on and below the horizontal plane through O. Just as in the Newtonian world, however, in Einstein's world the disturbing influences upon O can originate with events over which we have no control, of which we have no knowledge and for whose occurrence we would otherwise wish to allow. Since Einstein's shielding of O from effects due to events *simultaneous* with O does *not* shield it from the perturbational action of *earlier* events, Mr. Ushenko is mistaken in asserting that his analysis "entitles us to conclude that the cause O is completely isolated from interference." In fact, had he gone on to a consideration of Mach's principle in the theory of gravitation given by the *general* theory of relativity, then it would have become clear that our predictions are just as much threatened in that theory by the effects of unknown distant masses on the local metrical field as they are by Newtonian action-at-a-distance perturbations due to such masses.

While agreeing fully with Mr. Hofstadter that the validity of classical mechanics does *not* depend upon the existence of conservative systems and thus does not require isolability in that sense, it should be said in partial support of Mr. Ushenko that

(1) the issue is not whether isolability is required by classical mechanics but whether it is required in order that *causality* may prevail;

(2) Margenau has shown clearly [7] that if causality, as conceived in daily life and science, is to prevail in the world, then it is *necessary* that the laws of nature *which govern closed systems* are *not* explicit functions of the time. In the case of the one-dimensional motion considered by Mr. Hofstadter, this

[7] H. Margenau, *The Nature of Physical Reality* (New York, 1950), Ch. 19.

temporal invariability of the laws of nature entails that the mechanical system subject to such a law is conservative.[8] Consequently, in the case of the one-dimensional motion considered by Mr. Hofstadter, the constancy of the mechanical energy of the system is a necessary condition for *causality* though not, of course, for the validity of classical mechanics. Thus while Mr. Hofstadter observed correctly that "classical mechanics would still be true" even if "no single system were ever conservative," this observation misses the point, since the issue is what would happen to *causality* if "no single system were ever conservative";

(3) Mr. Hofstadter has overreached himself in claiming that Mr. Ushenko's introduction of a principle of isolability is both irrelevant and "unrepresentative of the physicist's meaning." This is shown by the simple example of the second law of thermodynamics which at least one authoritative book states as follows: "The entropy of a closed system never decreases."[9]

2. Another of Mr. Ushenko's principles of causality reads as follows: "Every event must have a cause." Since his theory emphasizes *singular* cases of causal compulsion, it is natural that we ask: Is this causal principle intended to affirm a connection between *all* of the characteristics of a given event and *all* of the characteristics of the event constituting its cause? Or does Mr. Ushenko confine himself to asserting a connection between subsets of the characteristics of these events in the manner of those who hold that causality is a relation between *kinds* of events? In order to have endorsed the former of these claims meaningfully, he would have had to solve the logical problem of determining or unambiguously stipulating the membership of the class of "all characteristics of event E," a problem which must be solved, incidentally, if the thesis of universal determinism is to receive a clear formulation.

He does not consider the alternative just presented but proceeds instead to give us the following curious definitions of the terms "cause" and "effect": "We define the cause as the initial state that sets a physical process going, i.e., the complex of initial or boundary conditions of which some are vectors. The effect is then defined as a subsequent state within the same process that happens to be next under observation." And he illustrates these definitions by reference to a particle moving *continuously* from

[8] *Ibid.*, pp. 405, 410–411.

[9] *Ibid.*, p. 216. See also Lindsay and Margenau, *op. cit.*, p. 216, and L. Page, *Introduction to Theoretical Physics* (New York, 1935), p. 282.

rest at $x = 0$ to $x = x_1$, where it arrives after 1 unit of time with velocity v_1 and where it is first observed after leaving its starting point.

I contend that these definitions constitute terminological evasions of a further difficulty in Mr. Ushenko's theory. Realizing that his theory of causal intelligibility requires the causal chain of states to consist of a *discrete* sequence in which the effect follows *consecutively* upon the cause, he thinks that he can ensure such consecutivity by the simple expedient of defining the effect of any cause as the state within the same process which "happens to be *next* under observation." But both classical mechanics and relativity theory—theories which Mr. Ushenko has otherwise been invoking to support his views while eschewing quantum mechanics—tell us that in the *continuous* motion of the particle, *no* push-pull consecutivity obtains between *any* two states of the particle. Furthermore, it is absurd within the context of these two theories to make the answer to the question of whether event B is the effect of event A depend upon the subjective accident of our happening to observe B next after observing A. Mr. Ushenko is, of course, entitled to his own definitions of the words "cause" and "effect," but it seems clear that the definitions he has chosen play the role of an *ad hoc* principle in his theory and that they land him in a morass of subjectivity, as Mr. Hofstadter has rightly contended. More significantly, since the physical theory on which he relies rules out push-pull consecutivity between causally connected states of a physical system, Mr. Ushenko is not entitled to use the term "must" in his sense of *contact compulsion,* when he says that "Every event must have a cause." It follows that he has not succeeded in giving a viable interpretation to this principle capable of supporting his theory of explanation in science.

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REMARKS ON CAUSATION AND COMPULSION[1]

PROFESSOR Ushenko and Professor Hofstadter have precipitated us into the middle of things: into a central region of philosophy and into a debate which seems to have its beginning and probably its end elsewhere. Their subject furthermore is splitting into two, the questions whether causal explanation must involve an ontological compulsion, and whether causal connections,

[1] Read in the symposium on "The Conception of Law in Science" at the 48th annual meeting of the American Philosophical Association, Eastern Division, at Bryn Mawr College, December 27, 1951.

compulsive or not, are subject to interference and exceptions. On both questions my heart goes out to Mr. Ushenko for his soaring affirmations, but my head is with Mr. Hofstadter's (and Mr. Grünbaum's) niggardly critique. Causation is at most regular sequence, but also it is at least regular sequence.

The reason Mr. Ushenko is satisfied with causes which evince less than perfect concomitance is that he thinks that every cause involves something more than any concomitance, and this is the proposition which I wish chiefly to question. It goes in the teeth of what I think the very pattern of philosophic wisdom, namely, "actualism" or "factualism." This is the thesis that the world is the totality of what is the case, a manifold or continuum of fact, a sort of four-dimensional marble cake, variegated with juxtaposed and strung-along complexes of occurrent characters. To know exactly how the manifold is mottled would be to know everything; to spy out part of this is the sole end of any inquiry. The cosmic cake might have been utterly homogeneous, brown and soft and chocolate-flavored, for example, at every point and every instant. It might have been a flickering chaos. In fact, however, it is our familiar mixture of regularity and diversity, which lends itself with a little coyness to our inductive research. In particular, we find in it the two patterns called "substance" and "causation." A substance or "thing" is a worm-shaped prolongation of rather uniform events through a considerable time. A causal connection, or chain, or cone, comprises a set of events (the effects) which are variously deployed in the manifold but generally in patterns which cut across substance-worms, and whose characters are correlated in assignable ways with a central or initiating event (the cause). Causation is exemplified indifferently by the cooking of a biscuit and in the astrophysics of relativity. But there is not any single solemn official type either of causation or of substance. They are two among many conceivable modes of regularity, *de facto* and approximate, by which we could infer the occupants of one world region from those of another.

Mr. Ushenko's theory of causal compulsion is ambiguous in the ways which Mr. Hofstadter mentions, and in others too. But mainly, I think, we should contrast the natural or actual elements in it with what I can only call the preternatural or non-actual ones. The natural elements include the quality of tension, strain, or pushiness, the envisaging of an end, the sense of one process "growing into" another, and the final aura of consummation and satisfaction. These are experienced actualities and fit intelligibly into the manifold of fact, but they have little or no philosophical significance. They probably are only "secondary qualities," not in-

herent in causal transactions generally but only in our sporadic perceptions of them; and in any case they are "epiphenomenal" in the sense that they are only regularly associated with the other components as those are associated with one another. It is, then, just an empirical generalization that a hammer's blow on a walnut is regularly followed by the nut's cracking *and* that the various phases of the sequence are regularly suffused with pushiness.

The preternatural or non-factual components recognizable in Mr. Ushenko's account are an Aristotelian potency or metaphysical vector or disposition, like a compressed but non-existent spring, in the cause term, and a modal relation analogous to logical entailment between the cause term and the effect term. These are hard to reconcile with the scheme of fact at all. Prime matter, transcendent universals, pure potencies, not only are unobservable (of this I would never complain) but cannot be conceived as part of what is the case. The only potency that can *be* consists of the actual determinate details of things which, when supplemented or surrounded by other but similarly determinate details, are in fact succeeded by certain detailed consequents. A little less obviously the scheme of fact seems also inhospitable toward anything like an entailment between cause and effect, for the same traits of it which enable one fact about one locus to entail another fact about the same locus, as *This is square* entails *This is rectangular,* and enables one locus to be similar or dissimilar to another, preclude that a fact about one locus should entail a fact about another locus. There surely can be no *transeunt* entailments in a manifold of existence and location.

The idea of the manifold itself, of course, is not beyond criticism. But it is a scheme of analysis and construction on a deeper level, and better certified, than any particular hypothesis, so close under the wing of logic and so integral to science that only the sharpest criticism and most monumental counter-construction could turn the scale in favor of a fundamentally opposed philosophy. If I am not mistaken, it is the conceptual frame of Mr. Hofstadter's remarks and of Mr. Grünbaum's, and even Mr. Ushenko must resort to it in order to avail himself of Einstein's world lines and causal cones.

Taking Mr. Ushenko's causal dispositions nevertheless at their face value, we can note some interesting problems. How, for example, can a causal state of affairs be said to contain a tendency toward what in fact it does not and cannot accomplish? Exactly the same constitution of gunpowder which makes it explode when it is dry and lit, makes it *not* explode when it is wet. Anything in fact will result in anything, given proper auxiliary conditions,

so that the theory of inherent dispositions must conceive every object or event as bristling like a metaphysical porcupine with vectors in all directions at once.

Mr. Ushenko has surely been aware of the rather shopworn objections which we bring against him. How does he propose to override them? He may be content that the relation of necessitation is empirically given; but the rest of us must rather suppose our colleague deluded than admit that he has veridical faculties of a wholly different order from ours. He does not use the plausible old argument from the problem of induction, but he does argue from the converse problem of explanation. A reputed cause, he contends, really "explains" an effect only if it involves real compulsion. Mr. Hofstadter and Mr. Grünbaum deny this, and on the main issue, again, I think they are right. The interest in explanation is at best derivative; what we want is *information* about the mottling of the manifold, and the sane man would prefer the most chaotic description of fact to the finest of false explanations. But the logical core of explanation is provided by extensional connections or "subsumptions" anyhow. The material truths that Calvin Coolidge was a rural New Englander and that there exist no rural New Englanders who are not conservative, entail and therefore explain that Calvin Coolidge was conservative. On the same pattern with this trivial instance are constructed the most subtle and inclusive systems of science and philosophy. Where we have this pattern, nothing else is needed for explanation; where we lack it, nothing else suffices. It involves an entailment, to be sure, from the case and law to the result, but not one from the cause to the effect.

Mr. Hofstadter and Mr. Grünbaum, however, are a little less sympathetic than need be. The cash value of an explanation is that it reconciles us to the explicandum. The mind with purely logical motivation finds this satisfaction in schemes which unite many diverse facts under relatively few rubrics. But we need not condemn those who are particularly pleased by explanations whose terms are especially familiar, as the common man is, nor those who love explanation by purposes or values, as Socrates did. So we need not censure Mr. Ushenko's sentiment in favor of causal situations suffused by the quality of compulsion. He goes wrong only if he supposes that any of these preferences is proof that the universe is prepared to satisfy it, or that this is any *logical* loss. An explanation which met these demands would be a "better explanation" but only in the sense in which a gold latchkey is a "better key." They are an explanation and a latchkey, respec-

tively, and they are better, but they do not explain or unlatch any better.

There remains an argument which is causing anxiety among even the most critical analysts: that the truth of subjunctive conditional statements such as "If we had operated earlier, he would still be alive" requires a causal connection stronger than generalized material implication. Mr. Hofstadter says that current efforts "point in the direction" of an extensional solution, but alas, they do so only as alchemy could be said to "point in the direction" of the transmutation of metals; for in fact, by common consent, they have failed. Transmutation was eventually realized, however, along rather different lines, and I have no more doubt than Mr. Hofstadter that these conditionals can be assimilated to the extensional scheme. Meantime, I can't share his reluctance to give the name "law" to the crude generalization that there are no immortal men, nor can I see that his substitute, *There are no immortal men who don't drink celestial champagne,* is an important improvement. A law, like the first, which is without exceptions except for such conditions as in fact do not occur, is a law without exceptions altogether. But Mr. Hofstadter's interest in decreasing the "accidentality" of our propositions is perfectly sound. It would improve their explanation by finding them cases or subordinate laws under very general laws, usually by taking account of the composition of apparently diverse things out of uniform components. With this principle of analysis and hierarchical generalization, and with a logical concept of inductive credibility, I think that subjunctive conditionals can be given all the significance they deserve—which is not very much. By the same token, however, the operation of the same apparatus, often unacknowledged, should be enough to account for the plausibility of the theory of necessary compulsion right down the line.[2]

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[2] The above paragraphs, presented in connection with Mr. Ushenko's original essay, remain pertinent, I think, to his revision. The latter puts less stress on the experience of pushes or propensities and more on some more technical devices. He may well be right (end of Sect. 1) that "necessity entails generality," but the question surely is whether the generalities of physical law entail necessity. I don't understand how a causal *law* can be a conjunction of a physical law and a particular causal state (end of the third paragraph), but nobody would dispute that such a conjunction entails the effect; if Mr. Ushenko has a distinctive thesis it is that the causal state itself entails or otherwise requires the effect, and no shift of language will establish that. His resort to physical tendencies and geodesics, and the replacement of short "events" by long "processes," seem to me ineffective because, as I see it, it does not matter how thick or how thin we slice the world bologna—every

BOOK REVIEWS

Modern Science and Modern Man. JAMES B. CONANT. New York: Columbia University Press, 1952. 111 pp. (Bampton Lectures in America, Number 5. Delivered at Columbia University 1952.) $2.25.

This attractively bound and attractively printed volume contains the Bampton Lectures at Columbia University for 1952. The intended scope of the lectures can best be given in the author's own words: "I propose to examine the cultural significance of what has been going on in science since, say, 1935. And by 'cultural' I have in mind the whole social pattern of Western civilization. I have in mind far more than the modes of production, distribution, and communication; I wish to include in my survey the impact of modern science on the philosophic presuppositions of the average enlightened citizen of a modern democracy—on his ambitions, his hopes, his fears, his outlook on the world. So in a sense I shall relate physics to philosophy, but only by handling both subjects in a general fashion and viewing each from the point of view of a deeply troubled modern man" (p. 5). The lectures are addressed, however, not to professional philosophers and scientists, but to the "proverbial college graduate—the hypothetical individual whom college presidents welcome each commencement to the fellowship of educated men" (p. 3). I shall attempt to remember that the author has deliberately imposed on himself this rather severe restriction.

The eminence of the author as scientist, educator, and administrator lends authority to the discussion in the first chapter, "Science and Technology in the Last Decade." Mr. Conant's thesis is that in the last few years we have witnessed the emergence of a new social phenomenon: we have seen the scientists becoming inventors. This is most dramatically obvious in the work of those scientists (and the author emphasizes that they were *scientists*) who developed the atomic bomb, but it is equally obvious in the contemporary interdependence of science and industry. I am not

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slice remains logically independent of every other. I despair of doing justice to the argument of his last three pages, which bears an interesting resemblance to Scholastic efforts to derive the law of sufficient reason from the law of non-contradiction. Most of it, however, I think, is circumvented by the explanation that when we of the opposing school say that F instead of B might have occurred between A and C, we mean that the nature of A and the nature of C and the fact that there is something between them (with all the rest of the universe thrown in, if you like) no more entail that the something is B than that it is F, and quite conceivably they might not even make the one any more probable than the other.

certain, however, whether Mr. Conant wishes to assert (1) that science is now indistinguishable from technology or merely (2) that an increasing number of individuals who are able to do science proper are spending most of their time doing technology. Without the assumption that science has now become technology, the following statement is unintelligible: ". . . when in World War II it came to mobilizing technologists to assist the military forces, there could be no question but that the scientists as scientists would be called on by the government" (p. 9). On the other hand we have such statements as this: "For most scientists, I think the justification of their work is to be found in the pure joy of its creativeness; the spirit which moves them is closely akin to the imaginative vision which inspires an artist" (p. 58). Furthermore there is no doubt that the author is "deeply troubled" by this new relation of science and society: ". . . one must ponder on the consequences of the vast sums of money now being spent on secret military research and development undertakings. One cannot help wondering how long a large fraction of our scientific manpower can be employed in this atypical scientific work without threatening the traditions that have made science possible" (p. 30). I conclude that Mr. Conant wishes to preserve the distinction between science and technology and that the new social phenomenon to which he refers is best described by proposition (2) above.

This first chapter also contains a certain amount of material which had already been presented much more fully and effectively in the author's previous volumes, *On Understanding Science* and *Science and Common Sense*. I might mention in particular Mr. Conant's apt criticism of the common tendency to talk about "*the* scientific method" and his distinction between "limited working hypotheses" and the "hypotheses on a grand scale" (p. 26) which are characteristic of genuine science. I must admit, however, that neither in the two previous books mentioned nor in the present volume have I found any *precise* distinction between a limited working hypothesis and a hypothesis on a grand scale, aside from the obvious fact that the former is deducible from the latter (with the aid perhaps of other assumptions).

The second chapter is entitled "The Changing Scientific Scene, 1900–1950." For reasons that are not at all clear to me Mr. Conant believes that "a revolutionary weapon and the dream of a revolutionary future source of industrial power have become thoroughly entangled with what some believe to be a revolution in man's concept of the universe" (p. 32). This latter revolution is, of course, the characteristic denial of quantum physics (accord-

ing to the Bohr-Heisenberg interpretation) that the propositions of quantum mechanics are to be taken as literal descriptions of an objectively real world. Mr. Conant considers that this is similar to the view of J. J. Thomson that "a theory of matter is a policy rather than a creed" (p. 53) and that it conforms to his own definition of science as "a series of interconnected concepts and conceptual schemes arising from experiment and observation and fruitful of further experiments and observations" (p. 54). He deduces the conclusion that this view that physical theory is a "policy" rather than a "creed" is incompatible with a "materialistic World Hypothesis" (pp. 98 ff.) and a naturalistic pessimism of the sort represented by Russell's *A Free Man's Worship* (pp. 82–83).

There is no evidence in this chapter, or elsewhere in the book, that Mr. Conant is drawing his own inferences from a first-hand study of quantum mechanics and contemporary cosmology. He prefers in fact to quote passages from the semi-popular writings of P. W. Bridgman, Pascual Jordan, Herbert Dingle, and E. T. Whittaker in stating what he considers to be the philosophical implications of contemporary physics. This method of presentation may, of course, be dictated by his intention to address himself to the "proverbial college graduate," but a practicing physicist, I suspect, would find his account derivative and unconvincing.

The last two chapters are entitled "Science and Human Conduct" and "Science and Spiritual Values." "I propose in this lecture and the next to see what, if any, significance the new interpretation of science has for the philosophy of life that guides the daily decisions of thoughtful men and women" (p. 61). This is the explicit intent of these last two chapters. In the third chapter the author proposes "to explore certain consequences of the revolution in physics in terms of their effect on human conduct" (p. 60). The thesis maintained in the chapter is this: "Scientific concepts are so much a part of the equipment of men and women in our culture that they are used both consciously and unconsciously in making decisions that we call ethical or moral. The activities of scientists in their laboratories are shot through with value judgments. Such at least is the twofold thesis I am defending now" (pp. 62–63). I am sure the common reader, as well as the professional philosopher, will find it difficult to discover the relevance of this thesis to the previous proposal. The characteristic concepts of quantum mechanics are ordinarily not called "commonsense ideas," and even if they were, it is certainly not true that they *are* used, nor is it easy to see how they *could* be used, in "the daily decisions of thoughtful men and women." Mr. Conant would re-

ply: "To my mind, any attempt to draw a sharp line between common-sense ideas and scientific concepts is not only impossible but unwise. Belief in the whole apparatus of a three-dimensional world and in the existence of other people is a policy essential for an individual's survival; for a physicist or chemist in his laboratory, a new working hypothesis is a policy guiding his conduct as an experimenter. Where is one to draw the line?" (p. 80). But surely the beliefs mentioned here are not peculiar to science. I should assume that they are held, for example, by a theologian. By Mr. Conant's argument it would follow that they should also be called "theological." Scientific concepts and beliefs are those which science does *not* share with the other disciplines nor with "common sense." Mr. Conant can demonstrate the first part of his twofold thesis, therefore, only by a confusion of categories.

No one, I suppose, will dispute the truth of the second half of the thesis. What is questionable is its relevance to the proposal to consider the effect of the revolution in physics on human conduct. From the assertion that the activities of scientists are "shot through with value judgments" (which spring from an "emotional bias") Mr. Conant deduces the necessity of finding experts with opposite emotional biases when policy decisions involving technical questions have to be made. It is possible that the emotional biases of the experts could be neutralized and balanced by this method, but does this mean that the administrator, entrusted with the policy decision, is now free to exercise his own "emotional bias"? Mr. Conant's proposal provides no safeguard against this contingency.

The general problem of the ultimate foundation of our value judgments and our attitude toward "spiritual values" is discussed in the last chapter, and Mr. Conant's position is that while science can aid in the realization of spiritual values it cannot "in principle" determine what our value judgments should be. With respect to the spiritual value represented by the imperative, "Help the suffering," he says: "If loving your neighbor as yourself is the epitome of a religious outlook, it can only have meaning as a policy to the extent that one is able to help the neighbor when he or she is in pain or trouble" (pp. 86–87). (The phrase "have meaning" is presumably intended to be synonymous with "be effective.") In this chapter Mr. Conant can use the conclusions of the second and his own conception of science to show that, since science is a policy rather than a creed, it cannot provide us with an inclusive "World Hypothesis" of the sort represented by dialectical materialism or classical physics. With respect to the whole question of "spiritual values" his conclusion is that "in

regard to many phenomena, we require, as men of action, at least provisional beliefs. These phenomena are open to scientific investigation, but until the degree of empiricism has been greatly lowered, we must operate with rules of conduct that are *not* based on science. This being so, I doubt if we can escape some attempt to put these rules into a conceptual scheme, however hazy. The evidence for the scheme is not affected one way or another by findings of the scientist, whether he be a physical scientist or social scientist. The evidence is based on personal experience and its prolongation by history" (pp. 106–107). These "minimal commitments" are considered to be "imbedded in a man's total personality" (p. 109), but it is implied that it is the business of science to reduce the "degree of empiricism" of these provisional beliefs and replace them by properly certified scientific statements, although this process of replacement can never be wholly successful. I doubt that this account of the relation of scientific statements to value judgments will appear convincing to the readers of this JOURNAL.

I found no typographical errors in the book, but I suspect the following sentence was not intended to be printed as it stands: "These [common-sense ideas] have come to us in large part as a result of trial and error, in part by experiences that cluster together and which we designate as other persons" (p. 63). There is no other passage in the volume to support such a radical phenomenalism.

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Symbolic Logic; an Introduction. FREDERIC BENTON FITCH. New York: The Ronald Press Company [1952]. x, 238 pp. $4.50.

In this book is presented a demonstrably consistent system of logic which, according to the author, "seems to be adequate for all of mathematics essential to the natural sciences." Apparently Chwistek made similar claims of consistency and adequacy for some of his systems. More recently, Paul Lorenzen also contends that he has proved the consistency of classical analysis, as well as the modern theory of measure and integration, from a non-axiomatic approach. In each of these cases, there are peculiar obstacles in the way of any attempt to determine whether such high claims are justified. With regard to Professor Fitch's system, I think the most difficult part is to convince oneself of its adequacy for mathematics or at least for "useful" mathematics.

Many highly interesting and original ideas are employed in the construction of the system. The author's prime innovation can best be illustrated by considering Russell's paradox of the class K of all classes which are not self-members. The class K, it will be recalled, belongs to K if and only if it does not belong to K, and since both cannot be true, there is, therefore, a contradiction. There are at least two ways of avoiding this paradox. The usual way, followed by both Russell (type theory) and Zermelo ("set theory"), is to deny the existence of any such class K. A different program, which is often suggested but, so far as I know, has never been carried out, is to admit such a class K but reject the following case of *tertium non datur:* "either K belongs to K or K does not belong to K." For example, Skolem (*Über einige Grundlagenfragen der Mathematik,* 1929, p. 20) suggests a rudimentary class theory which is demonstrably consistent and in which a class like K occurs but neither "K belongs to K" nor "K does not belong to K" is true. However, Skolem has not worked out the underlying logic of inference and his system as a theory of classes is rather weak. In Professor Fitch's book, we seem to have the first fairly thorough execution of the program.

Indeed, Professor Fitch considers it an important virtue of his system that in it self-referential propositions are meaningful and sometimes even provable. To safeguard consistency, he assumes the law of excluded middle only for "definite" propositions. Thus, for example, "K belongs to K" is not a definite proposition. The propositional calculus of the system is somewhat similar to Heyting's intuitionistic logic. But there are important differences. For example, although both systems lack the general law of excluded middle, the double negation of p implies p in Fitch but not in Heyting; "if p implies q then the negation of q implies the negation of p" and *reductio ad absurdum* are valid in Heyting but not in Fitch (except when the propositions are "definite").

To avoid the paradoxes, Professor Fitch uses, in addition to the restriction on *tertium non datur,* the following novel principle: a proposition p is not to be regarded as validly proved by a proof that makes essential use of the fact that some proposition other than p follows logically from p. Specifically this principle is introduced to avoid Curry's paradox which in ordinary logic would be a direct variant of Russell's paradox. Instead of the class K Curry considers a class K′ of all classes x such that if x belongs to x then (say) 2 plus 2 is 5 (in ordinary logic, this is equivalent to "x does not belong to x"). Suppose K′ belongs to K′. Then, by definition of K′, if K′ belongs to K′, then 2 plus 2 is 5. There-

fore, by assuming that K′ belongs to K′, we can deduce that 2 plus 2 is 5. Hence, we can prove (for example, by deduction theorem): if K′ belongs to K′ then 2 plus 2 is 5. But then, by definition of K′ again, K′ belongs to K′. Therefore, 2 plus 2 is 5. This derivation violates Fitch's new principle because the proof of "K′ belongs to K′" makes essential use of the fact that "2 plus 2 is 5" follows logically from "K′ belongs to K′." It turns out that this additional restriction enables Fitch to avoid not only Curry's paradox but all contradictory results, because his system thus restricted is proved to be consistent.

The system is in a certain sense nominalistic: two different non-abbreviational expressions are never to denote the same thing. In particular, two classes are regarded as different if they are denoted by different expressions (abstracts), regardless of whether or not they have the same members. Therefore the principle of extensionality is not true in the system. It would be interesting to know the extent to which the lack of this principle affects the derivation of ordinary mathematics.

Furthermore, it is assumed that all the entities dealt with by the system can be enumerated effectively because the totality of expressions employed can be enumerated and because the things designated can then be enumerated in the same way. The assumption of such enumeration makes it possible to include certain infinite rules which enable us to derive general conclusions from premisses about infinitely many special cases.

The book uses the method of natural deduction or subordinate proofs due to Gentzen and Jaskowski. The official reason for this is to simplify complicated proofs. However, it seems to me, the method of proof of the consistency of the system is also tied up rather closely with this method of deduction employed in the system.

The system is a modal logic in the sense that it also deals with logical necessity and logical possibility. However, the author has not done much with the modal concepts in his book. Indeed, the modal parts are so completely isolated from the rest of the book that the author explicitly states that those who are not interested in modal logics could skip such parts. Moreover, as we know, we can incorporate the modal concepts into most of the ordinary systems of logic. Hence, this is not a very peculiar thing about Professor Fitch's system.

There is one appendix in which it is indicated how the system of the book can be developed by using combinatory operators.

In the second appendix, the author's principal system S_1 is described and a proof of its consistency is sketched. The proof

calls for elaboration but will presumably be found correct or at least can be made to work with suitable adjustments. On the other hand, development of mathematics in the system is not included but is promised for a subsequent volume. Elsewhere the author has developed certain basic principles of mathematical analysis in somewhat different but related systems. The author now observes that the least upper bound theorem and the Heine-Borel theorem can be proved in S_1 by using essentially the following results. If a class z is said to be definite when everything either belongs to z or not, then the union and intersection of all members of a collection of definite classes are again definite classes. Similarly if a relation R is definite when for any two things R either holds between them or not, then the ancestral of R is also a definite relation.

Therefore, many basic theorems of analysis are presumably demonstrable in the system S_1 of the book. But such an achievement, in my opinion, will not yet provide a justification of the adequacy of his system. In particular, since Professor Fitch's basic logic of inference is very much different from the ordinary elementary logic, the fact that ordinary mathematics can mostly be derived from certain fundamental principles by ordinary logic and that analogues of these principles can be proved in his system will not assure us automatically of the derivability of mathematics by his logic from these analogues of the ordinary theorems.

The remaining appendix reprints an article of the author's on self-reference in philosophy. The author considers various kinds of self-reference in philosophy and in mathematical logic. Roughly speaking, the author divides forms of self-reference into two categories: those which are harmful since they lead to contradictions, and those which are desirable since they enable us to deal with all theories or all real numbers. According to Professor Fitch, the system of his book retains the desirable kinds of self-reference while rejecting the harmful; the theory of types in its various forms, on the other hand, excludes the sort of self-reference that is essential to philosophy.

Both Professor Fitch's position and his system are interesting. Nonetheless, I should like to say a few words in defense of the ramified theory of types. He criticizes the ramified theory of types on the ground that in it the real numbers can no longer represent a genuine maximum level. Yet, as we know, this is true of every consistent *formal* system by "Skolem's paradox." Since every consistent system has a denumerable model, we can construct by the diagonal procedure a real number which is different from all the real numbers of any given system. In particular,

this applies to Professor Fitch's system. Indeed, since he assumes an effective enumeration of all the entities dealt with by his system, we could define even more directly a real number not captured in his system. Just as there is no greatest positive integer, there is no formal system which contains *all* real numbers. On the other hand, if our task is to define enough real numbers for use in the natural sciences, some form of the ramified theory of types (for instance, using transfinite orders or levels) would, in my opinion, serve our purposes as well as any other known alternative which can be proved consistent now. In it we can also prove certain forms of such theorems as the least upper bound and the Heine-Borel.

The charge that the ramified theory of types is self-referentially inconsistent seems to depend essentially on the assumption that it is taken as an all-embracing universal theory. It is not necessary so to take it. Indeed, there is little reason to expect any system of logic whatsoever to be all-inclusive. It seems more reasonable to suppose that every time we speak of *all* things, *all* concepts, or *all* classes, there are some which are left out. I suspect that if and when the philosophy behind Professor Fitch's system is formulated as sharply as Russell publicized that behind his theory of types, we might be able to find even more objections against Professor Fitch's general philosophy than against Russell's. It is often more easy to express correctly a formal system than the philosophy behind it. Sometimes this is because the formal system does not express the philosophy adequately. Then we find the system objectionable because of the discrepancy. At other times a system may happen to be better than the philosophy behind it. There are also interesting systems behind which there is little articulate philosophy.

According to the author, the book is intended both as an introductory textbook and as a treatise on the foundations of logic. Yet in my opinion it would be more correct to call it an introduction to a special future treatise (*viz.*, the author's) on the foundations of mathematics. It is questionable whether anybody who is not interested in pursuing further developments of the author's system would find it more profitable to use this rather than some other book as an introductory text. Some of the more advanced parts of the book appear to be not so much portions of a complete and polished treatise, as a report of researches of the author currently going on.

Despite such apparent shortcomings as an introductory textbook, this is an extraordinarily original and stimulating piece of work. I think it would be fair to consider it a significant contri-

bution to the problem of reconstructing classical mathematics on a demonstrably consistent basis.

HAO WANG

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The Logic of Personality. BERNARD MAYO. London: Jonathan Cape [1952]. 188 pp. 10s. 6d.

If philosophers and their publishers were to accept sometime in the future a code of professional ethics one provision of which would make it mandatory that publications should be fitted, however scantily, with titles covering at least the essentials of the subject, it would become unnecessary, as in this instance, to advise the potential reader that he will look in vain in this book for treatments of logic and personality in the customary senses of these words. But this book is unusual for other reasons as well. It constitutes a treatise in which, with the armory of logical empiricism, some tenets of this school of thought are re-examined with a view towards correcting them sufficiently to allow for the recognition of "personal relations" as primary and irreducible to sense-data language.

Mayo travels rather lightly, unimpeded by footnotes, bibliography, index, or preface. In the text, we encounter only occasional references to Russell, Haldane, Kant, and Macmurray; otherwise psychologists and philosophers are mentioned as classes which (with the exception of Macmurray) have neglected the author's problem. What is this problem?

Personality, Mayo points out, is not open to investigation by psychologists because, as abstracting scientists, they can only discover properties, i.e., trait-descriptions of what individuals may have in common rather than what distinguishes them as unique personalities. Thus, if A writes a testimonial to B about C, it is still only A, and not B, who knows C, even though there are now two people, both A and B, who know some of C's personal characteristics. This brings Mayo to Russell's distinction between knowledge by acquaintance and knowledge by description. Neither kind, it turns out, can satisfy the requirements demanded by the peculiar uniqueness involved in the "personal relation." One surely need not argue with the author that to know someone is not at all the same thing as to know someone personally. Is this personal knowledge, then, of the type called "acquaintance" by Russell? Not so, because the referents of knowledge by acquaintance are sense-data and these, by definition, are too transitory to be the sort of thing with which we can become as "familiar" as we do become with people. Mayo's extended discussion

at this point could have benefited, perhaps, from Chisholm's distinction between pre-analytically and post-analytically given data of experience. Pre-analytically, people and things are surely "given" before any conscious inference from sense-data. Russell's distinction of sense-aspects from their thing-reference was introduced, however usefully, for the purpose of separating out relatively more certain "elements" of knowledge from others less so. Mayo's purpose, being of a different nature, is not strengthened by arguing Russell's analyses (in *The Problems of Philosophy*) which, in principle, can be coördinated to Mayo's "personal relations" every time such cognitive issues are raised as, e.g., in the misinterpretation of the expressed meaning of one individual by another.

The attempt, on the part of some philosophers, to single out cognitive elements of perception is baptized by the author as "the cognitive heresy":

> This is my deliberately abusive name for theories aimed at maintaining the distinction between "cognitive" and "emotional" aspects of perception. The distinction is untenable. "Emotional" elements, such as the sensation of familiarity, are required to supply the characteristic of permanence needed for any object of perceptual knowledge. [P. 43.]

Even if one were to agree, however, that because cognitive and emotional aspects occur together in any given act of perception they can, for this reason, not also be usefully separated by analysis, such admission would turn out a double-edged blade, damaging also to Mayo's thesis that personal relations must be regarded as so exclusively unique that no amount of (psychologically cognitive) descriptions can do justice to them in any way. One therefore wonders what philosophy can contribute to their understanding. If "the personal life is not talked about but lived" (p. 101), Wittgenstein's dictum comes to mind that "whereof one cannot speak, thereof one must be silent." This is not Mayo's conclusion. He is positive that both logical and artistic discourse can be meaningfully related to the personal life: art, as may be surmised, by using language evocatively; logic, as is not so obvious, by making explicit the nature of the relation involved in personal experience. The gist of it is formulated in this way:

> (1) For anything to have individuality, one condition must be fulfilled: it must stand in a personal relation to somebody;
>
> (2) For anything to have personality, a second condition must be fulfilled; not only must it stand in a personal relation to somebody but also it must stand in the converse relation to something, or, in other words, something must stand in a personal relation to it. [P. 116.]

Both individuality and personality are here defined in terms of the "personal relation" which itself is left formally undefined but presumably open to ostensive clarification. It seems possible to agree with Mayo that personalities are indeed distinct from other things in that they can enter into dynamic contexts of feeling and action, even though Whitehead, e.g., considered the possibility that this may not be their specific difference from anything whatever. It is more difficult to agree also that arguments for the recognition of this context should take the form of devoting one's major attention to such tenets of the recent (and not so recent) philosophical tradition as remain strictly neutral with regard to both the assumption and any possible use one may make of this dynamic context. Nor is much use made of it in this book. Finally, even if one were to look wih favor upon the author's excursions into the problems of knowledge by acquaintance and description, of sense-data, perception, myth, language, art, and ethics for the purpose of separating the unique from the typical, it is still doubtful that all this is relevant for a logic of "personality" and that a treatise, dealing with it all, constitutes a "logic" of personality.

We are left with the realization that, unfathomable to both sense-data and knowledge by description, there are personal relations within dynamic contexts of action and feeling, and that the assignment of personality to people involves the logical converse of the personal relation between them. With this much granted, we either ought to remain silently dynamic or, if talk we must, to use language for the evocation or direction of personal relations. Since a philosophy of silence is not advocated, nor an appeal for philosophers to act as poets, it would seem to follow that unless philosophers can make contact with the descriptive sciences of human behavior, they will indeed have to content themselves with Mayo's argument for a context in which the undescribable sort of personal relations can be lived but not described. I find it easy to agree with the author's candid assertion toward the end of the book that one may have expected him "to gather up the loose threads of the discussion into a tidy ball" and that he had "no finished theory to offer." One concludes with regret that the often exacting analytical labors of a sharp and honest intellect have here been spent in a manner so unrelated to the methodological issues of empirical inquiries into human personality that, at best, they can only point to a promising field for philosophical work to come.

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Art, Form, and Civilization. ERNEST MUNDT. Berkeley and Los Angeles: University of California Press, 1952. 246 pp. $3.75.

Mr. Mundt is a sculptor, and director of the California School of Fine Arts. His book is an attempt to make us aware of the need for a more unified way of life than that which we presently claim to enjoy. This is not, of course, a novel thesis, and Mr. Mundt develops it very little, but the points he makes are made clearly and simply, and they are refreshingly illustrated. Starting from the premise that modern man's life is not harmoniously integrated, we are led to envisage a "unified man," as he has sometimes existed in the past and may again exist in the future. The better life of "unified man" would be brought about by a fuller consciousness of artistic and religious values. This heightened awareness would counteract the over-intellectualization of experience that has attended the development of scientific activity. Instead of trying to make everything rational, "unified man" would perceive that the values that really count are not rational at all.

Mr. Mundt leads us on several ingeniously arranged and delightful historical excursions: we are shown the changing conceptions of artistic space—from Egypt to Greece, from Greece to Rome, on through to the marvellous spatial unity of the age of the Baroque. We are shown how sculptural attitudes toward bodily tensions have changed through time, as have painterly attitudes toward color. The author compares capitals and sculptured heads of the same epochs, so that we may see by their expressive similarity the pervasiveness of reigning styles. He analyzes the pouring vessel through the ages, and demonstrates the need for awareness, honesty, and humility in even so unpretentious an object. Through these brief peeks at past epochs, Mr. Mundt endeavors to explain how art can be used to organize and to vitalize human living, by beautifying our environment instead of uglifying it. Toward this end, his book may perform a useful pedagogical function.

The chief difficulty I find in this presentation is the notion that modern life suffers from an over-intellectualization that can be correlated with the rise of modern science. For the extreme rationality which the author finds in certain aspects of contemporary art is also to be found in Greek art, produced at a time when science was in a relatively rudimentary stage. Such rationality is more apparent in individual works of Greek art than it is in their interrelations, as is shown by the haphazard planning of Greek cities. Perhaps this reflects the Greek ability to achieve balance and harmony in individuals, but not in groups. And what the

Greeks failed to achieve in their lives, they may also have failed to achieve in their art. But on the other hand, what they succeeded in getting into their art may have been, not the actual quality of their times, but the quality of their aspirations. (Similarly, the variety and multiplicity of late Gothic art may reflect much less a pluralistic society than it does the longing for such a society.) Therefore the art of a period may indicate, not what that period thought to be actual, but what it thought to be desirable. It can represent a response to the future as well as to the present or past. Like its period, art may contain its own contradictions (or "contrasts," as it is now more fashionable to say). It can even be critical of its period, and so reflect it all the more precisely. We must therefore beware the exceedingly tenuous generalization that seeks to connect rationalism in art with rationalism in life or with some particular phase of science. What is actually involved is probably a good deal more complex.

It is also uncertain how useful art can be in fashioning the more unified society that Mr. Mundt finds desirable. The integrating powers of art are easy to overestimate. It is also, of course, debatable that a more unified society is at all to be desired. Fortunately the author of this book seems to have little of the nostalgia for medievalism that so grievously afflicts many of his contemporaries, although he apparently shares their faith in the integrating power of religion. As might have been expected, Mr. Mundt appeals, for justification of the status of mysticism, to Mr. Eddington, for justification of cultural unity to Mr. Mumford, and for justification of scientific unity to Mr. L. L. Whyte.

But I fear I have unjustly forced this book's modest intent. For it hardly pretends to be an exhaustive assessment of the means at our disposal for the betterment of our condition. Its aim, rather, is hortative, challenging us to create a social order in which art, even if it is not to be the major integrating agent, will at least be itself vitally integrated. Knowledge and appreciation of the past help us to this end, because the past teaches us how things can be done, and what it is not necessary to repeat. Just two centuries ago Thomas Young wrote, "The less we imitate the Ancients, we shall resemble them the more." The importance of a society's knowing how to utilize and criticize its cultural heritage is one of the things this excellent maxim leaves out of account.

MATTHEW LIPMAN

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L'Histoire et sa Philosophie. HENRI GOUHIER. Paris: Librairie Philosophique J. Vrin, 1952. 151 pp. (Problèmes et controverses.)

Professor Gouhier, some years ago, wrote a book entitled *La Philosophie et son Histoire.* A thirst for symmetry has apparently compelled him to write a work entitled *L'Histoire et sa Philosophie.* There appears little further justification for choosing this title, since the book touches but obliquely those general problems which have come to interest thinkers whose concern is with the philosophical issues raised by the subject-matter and practice of history. To the understanding of these problems, Professor Gouhier has made no great contribution here; at best he has suggested, but not developed, a way of regarding those characteristic utterances which collectively constitute the history of philosophy.

These utterances, the author suggests, may be addressed from two conveniently available points of view; namely, *le point de vue du système* and *le point de vue de l'évolution.* In accord with the former, a philosopher's utterances are seen to form a consistently intelligible structure of thought—a system, however, which is reconstructed often by abusing the historical order of assertion, and where the criterion of priority is logical rather than temporal. Thus, an astute systematizer can abstract into coherence a collection of utterances which (for example) could constitute a "cartésianisme plus cartésien que la pensée de René Descartes" (p. 12). This, in effect, is to discover the "essence" of a philosophy, eternalized and disabused of all biographical connections and—like essences generally—having no existence in time. The *other* point of view, that of evolution, looks to the emergence in time of a philosopher's utterances in the context of his historical location, provoked more often by the exigencies of existence than by the urgencies of systematic elegance.

But, the author suggests, neither point of view stands alone as sufficient ingress to a philosopher's thought. For, reconstructively to regard a philosophy as an essence rather than event, to eternalize some thinker's utterances, is unsatisfactory if only because many of these can become intelligible only in the light of particular historical motivations: much of Descartes makes sense only within the context of seventeenth-century knowledge, problems, and aspirations. Still, even the historical point of view presupposes the system, for it is the system which guides us to the relevant events: the autobiographical passages in the *Discours* tell of events which have retrospectively become meaningful to Descartes in terms of the system of thought which they helped to produce: the biography of a philosopher is as much a product of his system as his system

is a product of his life: "C'est le spinozisme qui oblige Spinoza à préférer le polissage des verres à une chaire d'Université" (p. 93).

The great systems of philosophy have an odd durability, communicating something meaningful to each generation, being returned to recurrently and perennially. But not every utterance of the great philosopher is of some eternal truth: to treat each of his statements as though it were so would be absurd. But to consider only those utterances which qualify for eternity would be to delete the mass of peripheral insight, the rind of gossip, which makes the history of philosophy the absorbingly human affair that it is. Let us consider these utterances then, the author urges us, as constituting *visions du monde:* this allows for everything, the eternal truth and merely historically interesting utterance. And just as Professor Gouhier's book becomes interesting, it ends. For we want to know what a *vision du monde* is. How does the characteristically philosophical *vision du monde* differ, say, from the scientific *Weltanschauung;* and how does one distinguish it from the artistic visions of Shakespeare and Shelley? Professor Gouhier tells us there *is* a difference, but he does not tell us what it is.

Had the author probed the structure and purport of the philosophical *vision du monde,* instead of exploring diffusely a problem which is no problem to speak of, he might have accomplished a useful task: the task, namely, of showing what a philosophical utterance is when it is neither a scientific statement nor an artistic expression.

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NEW BOOKS

(Books listed here may be reviewed later)

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NOTES AND NEWS

Herbert W. Schneider, Professor of Philosophy at Columbia University and an editor of this Journal, has been appointed to serve for two years as the head of the Division of Philosophy and the Humanities within the Department of Cultural Activities, which is one of five departments of UNESCO. His duties are to begin April 1, 1953.